The Harveian Oration

ON

THE PASSING OF MORBID ANATOMY.

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BY

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Twenty-seven years ago, in delivering the Bradshaw Lecture before our College, I stood in the place of a much regretted Fellow dead in his early prime, when, in a retrospect of his work on arterial tension, I had the more easy task of recalling still recent memories. To-day, when in pursuance of the honourable duty that you, Sir, in your kindness, have imposed upon me, I try to look back into the mists of centuries to the full life and ever-to-be-revered memory of Harvey, no longer can one appeal to those personal recollections in which we all could share and sympathize; in place thereof is an influence, a resurrection, and my outlook becomes essentially *pro*spective.*

Harvey's influence as it is permeating the avenues of time! What a subject, what an opportunity! Yet, when some 256 years have come and gone since this annual commemoration of our benefactors was instituted by Harvey himself, what an impossibility, in any but the most spectral form!

May I try to present it to you thus, on the wings of a thought—the passing of morbid anatomy—as it flits across the horizon of pathology, and in so doing touches us and humanity at large? No doubt it has often been said or thought before that, apart altogether from the coming into being of any particular man, the circulation of the blood, even to the full extent of Harvey's demonstrations, must have become common knowledge sooner or later. Even so, in his time, with anaesthetics still in the womb of the far-distant future, with instruments of precision hardly in their infancy, what a magnificent triumph of patient and determined observation was his! and it does but enhance the reverence we owe to him to add that the truest estimate of his fame rests even less perhaps on what he did than on how he did it, less on how he *did* it than on how he *said* it, or showed that he had worked it out in thought. It is the completeness of his work that is so surprising, and it is the plan of campaign that Harvey carried out by searching out the secrets of Nature with such deliberate self-content, that will as a motive power stretch further along the ages even than the actual conditions that he demonstrated, great as these truly were, and are.

Harvey taught us how to work and what to work at, and showed that the knowledge of the structure of our tissues goes far to suggest their functions. By actual investigation, by experiment, by what now goes by the name of "original research," and the reasoning and suggestion that emanate therefrom, the anatomy and even the larger details of the physiology of the circulation were worked out, and in the absence of any adequate powers of magnification, discovery in that direction must then have seemed to have reached its furthest limit. Yet who knows but what Harvey himself—physiologist, physician, and pathologist as he was—foresaw, or at any rate had some ideas about what the histology of the future was to tell? The book itself had its leaves uncut, but could such a mind as that altogether still the passion of the seer? He saw and taught that the knowledge of the skeletal tissues must come first; that this would open up some of the more pronounced changes of morbid anatomy, and their association with certain signs and symptoms of disease. Next, as opportunity developed, would come an inquiry into the minute structure of the normal tissues, and not till then would it be possible to interpret those finer alterations present in disease, or gain any appreciable knowledge of the subtleties of function. Such is, indeed, a sequence of events that has actually come about, and with which we are all familiar. Within the short span of our own lives this is true for our own time, so let me dwell to-day on *our* earlier pathology, trace it as now, and prospect its future.

And no more pertinent introduction could I take than the history of the growth of our knowledge of malignant tumours.

Years ago, when the scientific world was pulsating under the influence of the master mind of Virchow and of his cellular pathology, many of you, my learned colleagues, kindly here to-day, were hard at work studying and describing the features of morbid growths and trying to obtain an insight from the histological appearances of how and perhaps why they came.

What did morbid anatomy and histology—the cellular pathology of that day—tell to the mid-Victorian pathologist? Well! it was known in those now seemingly far-off days that the *auto*genous propagation of tumours was not uncommon; one could see it almost any day in the *post-mortem* room—in the peritoneum, for example, where a tumour originating in one part of that membrane was transplanted to another part, and was well rooted there. It was well known, as the result of experiment, that *exo*genous plants in similar regard were seldom attended with success. Yet one noted that when sowing—skin seed, shall I call it?—upon large sores, some stimulus was forthcoming; something happened that clearly set epithelium growing, and it often grew with considerable energy. This would apply to autogenous as well as exogenous grafts within the common species. I have always doubted if the import of that suggestion of skin grafting has received quite its just meed of recognition in its physiological aspect. The observation is full of instruction when we try to trace the life-history of the living cell, and still more attempt to influence the progress of its nutrition, its growth—even its very nature, perhaps—by stimulant or inhibitant, as is the aim, I take it, of the various grafting or tincturing processes by serum now in vogue. Again we knew, or thought we knew, that seats of worry were prone to take on malignant growth; that the well-known nag of a clay pipe, of irritating smoke, of soot, was associated with epithelioma; we knew, or thought we knew, that simple and malignant tumours were liable to be found side by side in the same body—a point that I have repeatedly demonstrated in the deadhouse, and that seems to me to cast a doubt upon the claim still maintained (I do not say wrongly so) that malignancy is something altogether apart from ordinary growth. But it was surmised by some, from what we saw, that a spermatic or, perhaps, graft-like influence was at work in the one group that was not to be seen in the other, and that, so it seemed to me, this influence gradually communicated itself to the cell structures in the neighbourhood until they ultimately shared in the process, and fraternized with the ill-tutored agitator. In this one respect, I may say in passing, the opinion then hazarded is not in accord with more recent work, which, on the contrary, goes to show that malignancy, even to its latest stages, is self-ordered and self-centred, and does not give a pattern to the surrounding elements. And yet, on the other hand, there is a suggestion of some more intimate relationship between the invaders and invaded, even in our *latest* information, for it would seem that the more matured tissues in which a new growth finds itself—the old hands, so to speak—may possibly exercise a very effective restraint upon the young bloods, and thus preserve the ordered going of the locality, by the drastic method of starving the newcomers out.

As one thought on these things, who could do otherwise than peer into the future, and in doing so I wrote thirty-five years ago, "What a subject for Darwin would be the cells of a cancer, if only they were tangible, if only we could observe the variations of tumours under judicious cultivation!"

I have often said during these many years of search for an exogenous germ of cancer, that I shall be disappointed if we find one, for malignancy, it may be only upon adequate environic invitation, already finds and fills its place as a *sine quâ non* of evolution.

One likened the position then to that of the chemist,

* This address was written in all essential details several months ago. To those who have read Professor Schäfer's recent utterance at the British Association it will be obvious that this hovers round a similar line of thought. So much did this seem to me to be the case that I had to ask myself the question, "Is it well to deliver it?" But there was no time to do otherwise; and, after all, it is based upon the ponderings of a lifetime, so I have altered nothing material.

who, putting together his scheme of elements, finds bits of his puzzle wanting; but, knowing what their form should be, he proceeds to look for them, finds them and fits them in, the whole one purpose and one picture.

And thus regarding Nature, is there anything improbable in such a pathology of morbid growths? Is there not in the natural order of things, if so I may speak of what in the empire of the corpus becomes disorder, a place ready for it—a chance that such a thing may come? Are we wrong to expect that if a complex body is possessed of energy, or many centres of energy for orderly growth and development, that this disposition or force will now and then slip its leash, and run off on its own account? An analogy of this kind seems to me to convey a workable conception of a scheme of malignancy, where ordered growth diverges by successive steps of variation, of indulgence of function, until cancer appears, until malignancy becomes the insanity, shall I say, of function. And does not an idea of this kind foreshadow, and father too, the modern doctrine of the individuality of protoplasm which I take to be taught by cell eating (phagocytosis) and other forms of irritative response of which we now hear and think so much?

The last effort of the friend of many here, the late Sir Henry Butlin, who was at work with us in the days that I recall—embodying as it did the results of thirty-five years of research and thought in this very region—surely all pointed to some such conclusion in his mind, for, although his introduction of the term "parasite" is liable to misinterpretation, the parasitic cell was no other than some derivative of a protoplasmic human cell, and thus was parasitic, if at all, only in behaviour, to which the highest organisms must plead guilty—for are we not all of us, at one time or another, sponges on the world we live in? And, regarding the living animal from the point of view of individualistic protoplasm, are some of the sports of our gardens so utterly divergent genetically from tumours of natural tissue, from the infective granulomata, or from the growth of a cancer? I should call the difference one of degree of vigour of growth rather than of kind. Exuberant energy will out. Think of the fact that *x* rays so stimulate the surfaces as to lead in the first instance to an excitement and proliferation of the superficial cells of the skin, and then too often to cancer. A most vital acquisition this to our knowledge—an obvious signpost, not too distinct, upon the road we are travelling.

Here life from two worlds is perhaps forming into line, and as in the one, so in the other, say by some direct inducement to the cell or adaptive alteration of the nutrient juices—photo- or chemo-synthetic in kind, may be—an intensive cultivation is brought about; one may even suggest that the equivalent of a top mulch is applied.

Thus, I see in cancer a home-grown weed or a prodigal son, may be—alas! by no means yet worthy of the fatted calf—and in my study I seem to light upon the fundamentals of eugenics, and, indeed, to hark back to their earliest dawn.

But, however this may be, the trend of all recent work carried out by that splendid Imperial Cancer Research Fund realizes the dream of the bygone pathologist, for a large part of the monumental labours of Dr. Bashford and his fellow workers, during the last ten years, although showing that *exo*genesis is possible, and defining its limitations, has gone to point the fundamental necessity of the study of the life-history of the individual cancer cell, and, as you know, this has now been done and followed up in short-lived animals (the mouse), where alone it is possible, the results seeming to show, not that the environment, but that the individual cell, is the master of the situation, and that the dominant issue that comes up is, What is there in that cell, or influencing it, that gives to it such an inexhaustible power of propagation—whence comes that boisterous energy of which I have spoken?

So in this quarter of study, as in others I must mention, we seem to have come to what is still but a mirage, to an inaccessible land, where histology perhaps may no further help, and where organic chemistry and physics must take up the running, and show what part they play in the origin of life; or, if not that, we must search for and find the homologue of cancer in some still more embryonic stage of existence and inquire of its habits there.

Thus what we call pathology is growing up. But are we sufficiently alive to the fact that it is no series of stationary phenomena, but constantly on the move, like all else in Nature, and in being so it shifts its ground? That it changes absolutely you will, I think, readily admit. As I think of disease in the *post-mortem* room forty years ago, what alterations have we not seen! Pyaemia may be said to be wiped out; typhus is well-nigh forgotten; typhoid fever has altered; diphtheria seldom attains the initial severity that so often characterized it of yore, and is much more amenable to our attacks; scarlatina is of a much milder type; erysipelas is more of a rarity; malaria and Malta fever have been run to earth; the late results of syphilis seem to me to be far less often in evidence; lardaceous disease, so very common in our early days, is now seen but seldom; and we have come at grips with acute rheumatism, and, let us hope, with tuberculosis. Probably as much might be said of other diseases, and all will, I suppose, admit that good old age is both more prevalent and enjoyable.

It is true that these ills belong to the great group of epiphytic diseases that has been abolished in direct response to the researches of Pasteur and Lister, and that there is no such evidence of any general move all along the line, but make away with even one large group of maladies, and how large a part of the morbid anatomy of the organs must alter too, and even then how little account would be taken of numberless arrested purposes that make for the beginnings of other diseases.

Pathology not only changes but it shifts its ground when we really pursue it. There is no more striking confirmation of this than a comparison of the practice of the Pathological Society of London forty years ago and now. Our meetings then were crowded with specimens of all kinds; a set discussion came only now and then, as a sort of relish in the genoral menu. *Now*, I am told, morbid anatomy is much less in evidence; we have left all that behind, and discussions of the intricate problems that are behind it occupy the attention of our members. Whereas we used to display disease in the gross—the results of disease, that is—we are now able to recognize finer changes, and more minute evidences of bioplasmic unrest that are behind those results, and these are the goal that we make for now. I say that these results must long ago have been suspected. I remember in the early days of ovariotomy making the observation that those who succumbed therefrom were mostly those who had suffered from haemorrhage into the parts concerned, especially, of course, into the peritoneal cavity. And examining the blood extravasated under these circumstances one could see it peopled with countless hosts of minute bodies that the bacteriologist is now in some measure able to name and to classify. It was evident in the rude experiments of those days that blood was a favourable soil for their production, and one even had a suspicion that intensive cultivation lay there, but failed to detect its presence.

And pathology still is shifting! We have not yet reached finality. Even bacteria are probably results and not causes; they strive, or cancel with one another, to ulterior ends, and we are gliding on in advance of the most painstaking morbid anatomy. As I have just now suggested for cancer: look how near the chemist and biologist have come. I struck a hybrid the other day—a bio-chemist! In devoting himself more and more to the minutest germs, and particles of protoplasm, the biologist is not far off the investigation of gases, and the chemist in concentrating his attention on gases, is apparently overlapping pure physics, which seems to have established, that many media supposed to be gaseous are really clouds of solid particles of inconceivable minuteness, borne along by a velocity of which no words can convey any adequate conception. It seems, therefore, not altogether within the realm of fancy to suggest that we may all be working now in some intermediate zone, on the confines between the three kingdoms of living energy—for the colloidal states of inorganic life, the crystalloids of biology, may perhaps allow of this expression of the facts—whose language at present we do not understand, and whose purposes or meanings we are therefore unable to interpret. I wonder!

Animate or inanimate we are never outside life. Montaigne puts it very quaintly when he says, "Even as Nature makes us to see that many dead things have yet certain secret relations to life." How like is radium, in all but death, to what I would call the aureole of life. Here seems to be one substance that is always spending yet is never spent; that has power within it to regather of its loss, and by its action on the cell may even be said to originate the function of vitality. But does the spirit of life die? It may correlate with other forces of Nature; it may perhaps transform itself to other forms of being; may it be that we begin to see how it might transcend the firmament of space? The physician thinks not of death, but of the tenacity of life, and of how long, save by catastrophe, it takes to bring the machine to a standstill.

Even in the presence of the pathologist death may be disputed. Many years ago there was what may be called a pathological axiom, that the pus cell was a dead cell. Yet it was at the same time evident that unemployable or even obnoxious as regards its influence in the home, it had colonizing proclivities that were charged with great possibilities in some new world. Dead indeed it might be from one point of view, yet from another one might say of it, as Lord Curzon said once of the British Empire: "It is not a moribund organism; it is yet in its youth, and has in it the vitality of an unexhausted purpose." Where, indeed, is the demarcation in a degenerate cell—we call it so from a narrow, self-centred aspect—between life and death? We know of none, for even desiccated it does but pass into some more minute state of atomic subdivision, that is almost certainly capable of renewed energy, although it may not as yet be within our ken to give an answer to the question, "With what body does it come?"

One may change, but one does not die.

All *living* things are rhythmic. Is it altogether otherwise with matter devoid of life? Within the cadences of that rhythm are the moan of disease and the pause and silence of death, but the *Resurgam* of life is ever the dominant of the song. And this is well illustrated by our growing appreciation of the activity of the pus cell—*Laudable* pus, as it used to be called, by a seeming contradiction in terms. I suppose on the altruistic plea that somewhere deep down in the worst of criminals there lies a warrant of nobility.

I know not whether now it has still further lost, or regained a character, when, reappearing as the leucocyte of to-day, as hungry chickens rush for food, it revels in cannibalistic orgies; or else, as some may prefer, it fulfils the more useful if more subservient function of scavenger, or destructor to the community. Then, as my dreams suggest, reinforced by such omnivorous gatherings, by ionization, shall I say, amalgam, symbiotic fusion or combine, these lusty atoms, disintegrating and ambushing into still more minute particles, rearrange themselves within the confines of the body politic in which they find themselves, and thus perhaps reincarnate some of the bacilli, cocci, and such other early and unstable existences as Nature seems to have in store for us for good and ill within the cycle of living things. It seems, indeed, quite within the possibilities of the revelations of the future that germ and sperm of a sort are here at work, and that a primer lesson is thus conveyed not alone on the origin of communities but also on the very beginnings of existence.

Now all these changes and shiftings of ground suggest to me the existence of a struggle on the part of the pathologist to find out some morbid change for every disease, and it is indeed admitted that we have been ever more and more reducing function to terms of structural change. But now the time has come when it seems worth while to insist that all this work upon the foundations — whether it be anatomical, histological, chemical, physical—has, unconsciously, perhaps, but none the less inevitably, been ushering in another point of view than that from which we started, and the latest phase of pathology is this more intricate one that concerns itself with the investigation of function. It is now no longer so much the morbid change as the prejudicial function that is to be our quarry, or rather the passage of the one into the other. It is to this point that the researches on cancer have come. As pathologists we are now trying to identify as it comes and passes that coign of clinical medicine—

> When that which drew from out the boundless deep
> Turns again home. . . .

Where a detrimental method of function commences devolution and sets towards decay.

And what numberless instances are there where, with no accessible morbid changes, this is the problem. Perhaps it may even be said that there is no disease, however much we think we know about it, in which this is not the problem still.

Take this latter case first, as exemplified by arterial tension (*De motu sanguinis*). It came first into more precise recognition in clinical medicine as a symptom of what we have known as a form of Bright's disease—a disease that is of structural change. But before very long it was noticed that it was present under conditions when the examination after death showed no alterations in the kidney of any moment. So Bright's disease has been re-christened, and now we hardly hear of it. That postal address is now "not known," and you must inquire for arterial sclerosis. This disease is constantly being brought under our notice. Why so? Because particular signs are supposed to betoken it. But do they? Would it not be more true to say that they are its occasional cause? The point is that the search for a structural change has shifted its ground on to a functional disturbance—quite properly, as I think—and then, incorrectly, the arterial state has come to be regarded as due to the condition (sclerosis) of which it is in all probability the cause.

Then *de motu cordis*. Much work has been done of late upon the irregularities of the cardiac muscle. This work is well known to you, and is of the very best, and not by a breath would I belittle it, for it is no doubt gradually emancipating a difficult subject of research from the mists of ignorance that envelop it, and by true Harveian method. But I may perhaps be permitted to say as a caution that *it is possible* to attribute to *disease* of the muscle more importance than by right belongs to it, and I believe it will be accepted in the future, as the result of further clinical observation, that many a case of arrhythmia where we now seem to be inclining to muscular disease, is in truth an aberration of the higher cardiac centres, and as such no true example of pathological anatomy.*

Diabetes shall be my between-maid—in part functional, in part perhaps otherwise. What morbid anatomy have we not sought in this disease, and have not found it? Long, indeed, have many of us hovered over the alterations we had *hoped* to find, and have not found them—changes in the nervous system, in the pancreas, in the liver, in the blood, and so on.

To my mind there is no more splendid, and yet pathetic, figure in the whole range of medical history than that of Pavy. In early life he set himself to unravel the pathology of diabetes, and in so doing he planted himself with correctly forecasting eye at the spot that made victory certain, and, to my mind, he won it, by taking the first parallel. But it may be doubted if he himself quite realized that he had done so, and there are those to-day who certainly think that he was defeated. I am not of that opinion. It has always seemed obvious, when one considers how greatly the output of sugar varies even under the most rigid system of dieting, that the excreted sugar could not be a mere output of what has been taken in; that the human kiln has, in fact, the power of in some way coining sugar out of its own constituent elements; and I believe that Pavy's ultimate credit for a real step onward in this still most intricate disease will rest upon his observation that the protein molecule is in some way split up and that a carbohydrate molecule emerges therefrom. Still other parallels need assault. The question has now to be answered, What causes this seemingly inveterate sugar craze on the part of the proteins? This curious reversion—may we call it?—to a plant-like metabolism in thus compounding with carbon to the rejection of its natural affinities. Is it nervous incoordination, or toxic disorganization, or what? Again we seem to be driven back, cancer-like, upon a loquacity of function, upon some misapplied vital energy behind the scenes, with which as yet we cannot grapple.

* Catarrhal jaundice is for me another remarkable instance in point. We have always been so hard up for a morbid change in this malady that we have come to accept as sufficient the existence of a plug of mucin more or less natural to the part. But can it be that the proper performance of a function, so important as the passage of bile, is in danger of being arrested by an everyday circumstance such as this? Some temporary check to the driving power seems much more probable. The old ideas of splanchnic melancholia and abdominal megrims were not without warrant, and indeed they are worthy of rehabilitation.

Who, till within quite recent times, would have thought that a protein molecule could give birth to sugar! I trust there is no chemist here to-day. To one of a prim age it seems quite a surprising outcome, yet I understand that bio-chemistry is already quite familiar with it, and regards it as an everyday occurrence, whether in the physiology of plants or animals. And how this must enlarge one's horizon of the subtilty of energy that lies within the human kiln! The mills of God, the engines of Life, are indeed wonderful examples of an indomitably complete performance. They seem able to enforce within the organic confines of the body that extreme atomic disintegration we are now learning to recognize in the inorganic world, and which seems to suggest that even the stability of the primary elements, as they were wont to be called, is not immutable—that our gold, for instance, at some future geological day may lose its caste, and rank as some inferior substance.

Brought about under such conditions of continuous performance no wonder that our reactions are thus delicate and complete—not that we manufacture something out of nothing, as might seem to be the outcome of my argument, but that by such means a much greater and more absolute resolution of ourselves is possible, and our primary elements being unfettered renew their potential energy, to synthesize again by some of Nature's seductively elastic methods as now still unknown or but guessed at. There is nothing strange in this. Our environment in diverse ways known and unknown is essential to the working of the machine, of course, but I am contending for the possibility of *still more direct* methods of combination that have become conceivable in these days of progressive physical knowledge. And from this point of view it becomes legitimate perhaps to try and catch a glimpse behind the veil of present attainment of the working of the obviously potent influence of climate upon temperament, constitution, and disease. For it is at any rate conceivable that light or electricity split up, energy in one garb or another liberated in this way or that, might make to waver between negative and positive some weak-kneed member of our society of constituents; might loosen the force of attraction that had hitherto kept its atoms bound; might thus katabolize our tissues, and evolve new varieties of function, in some hitherto unsuspected way. How, for instance, should plant life have photo-synthetic metabolism all to itself?

It is to points like these—unsubstantial, even visionary, though they seem—that pathology is now compelled to turn its attention. And in so doing, although there is still much to be done in the *post-mortem* room, it is leaving its terra firma of morbid anatomy (I say it with lingering regret) mapped out so well by the great masters of the past—by Baillie, Bright, Addison, Peacock, Bristowe, Jenner, Wilks, Payne, and many another still happily with us—and seems to be betaking itself to aviation—a deadly dangerous, albeit, as it must be, an earnest pursuit. Let us remember, lest we forget. And to the many pathological phenomena that I think emphasize this still more, and which I would fain call unequivocally functional—in their origin, at any rate—I must yet mention one or two.

It used to be said that acute disease is mostly an outburst from a chronic one. This is but very imperfectly true. It is true that many an acute attack of disease is *supposed* to be *de novo*, when, as a fact, further investigation proves that it has followed upon pre-existing changes. But then these in their turn have come about in so insidious a manner that they have never been capable of recognition as disease at all, and I am here to-day to contend that the future of pathology will be a physiological one, the study of erratic function that has confirmed itself into an habitually baneful one. And I see no reason for limiting the pathology thus engendered to function only. It is surely permissible to hold that morbid function may bring about structural change. There is not an organ of the body of which this is not true, but how largely and obviously so when applied to the brain! A large part of the structural disease of the brain is led up to by slow and insidious misapplications of thought and action that ultimately declare themselves as pronounced disease, insanity thus becoming a cancer-like mood of the cells of thought, as well as a mood of growth.

Another instance may well be fatigue, for *it* nowadays is a disease. What more common than to meet a woman or man who is "So tired, always tired"?

We all know the machine that will not spark aright. Every movement an effort; even rest a pain. You know too well that there is nothing to be called structural change; and that even rest furnishes you with no remedy. It is clear that what is required is a fresh stock of some form of energy for charging up the machine that we are as yet not able to supply. So far away, and yet perhaps so nigh. These are they who are born out of due time; the day of their like has yet to come. And with fatigue might well be coupled pain, for there are those who almost seem to be born in pain, and of a sort that no remedy seems able to assuage. Says the poet:

> The sweetest thing in all the world is pain
> Consoled by love.

But that is *extra medicinam*. Even more than death, pain is our hereditary foe; no quarter given. In olden days hysteria stalked abroad, but to-day a broader view of physiology would teach that pain is no certain indication of any existing morbid anatomy; that its intensity is always subjective, individual, aloof from standardization; and it is evident that in such chronic pain one needs to recreate the nervous elements, or to instil them with some electroid that shall reset the spring of the machinery in motion, and guide it smoothly on its livelong bent. Nor, though it has been the quest of aeons, does this seem to be inexorably beyond the knowledge of the future.

Of other groups of disease that might be mentioned few are of more abiding interest than that which concerns itself again with Harvey's realm, and the finer processes of the circulation. How little we as yet know about these conditions; but, surely, distraught function is busy with them. Think what suggestions are contained in such an hypothesis as that of vascular spasm, or, to put it more generally, of peripheral stasis. Long years ago our trusty and well-beloved Fellow, now Regius Professor of Medicine at Cambridge, wrote a short paper on mental anxiety as a cause of granular kidney. It was to me one of those illuminating suggestions that have added interest to my life. I believe it to be abundantly true, as I do that similar malign influences, by dislocation in some way, as I suppose, of our correlated impulses, make for cancer. You must have often seen the nervous, anxious, worried man with the phenomena of high tension, and have felt able to predict *in posse* the future disease of this organ or of that. Such conditions, real diseases though they be, are but functional; but what a wealth of pathology is wrapped up in them!

Thus, as it is with life, so in a measure is it with its diseases. Life itself is a developing function; it comes to each one of us from the past, with innumerable atomic differences gathered by the way, and it passes on to each of our successors similarly changing. So that Professor Arthur Keith, from another place, can say to us that a thousand years is as one day, so like is this to that; yet also one day is as a thousand years, and in that day so multitudinous and infinite have been the changes, that life *now*, is another life—the same, yet not the same.

This all too brief survey must suffice to show how much pathology there is yet in front of us if the future is to be disentailed of its heritage of disease, and how difficult it must be to acquire the information necessary to enable us to bring the aid we would to sick humanity.

The outcome seems clearly to be: Experiment, and ever more experiment. We come, indeed, back to Harvey's teaching: to search out the secrets of Nature by such means. But to that word "experiment" attaches no narrow meaning, and it is to humanity at large that I would now say that it is quite as much your duty as far as in you lies, as it is that of special scientific inquiry, to search out thus by way of experiment those secrets of Nature that are seemingly so elusive.

Look a little closer into this matter O man of the world. To give an illustration that may appeal to you. To touch effectively that liver of which we are all so tenderly conscious, and regard with such blind solicitude, we have to deal with an organ that is occupied in *coining the energy of its life* out of elements organic and inorganic, that are, for it, as yet, outside life. The continuity of life, therefore, itself becomes an experiment. Will this grouping of substances, this bio-chemical combination come off, or will the individual atoms fail to synthesize, and fall back again

into the sea of odds and ends still waiting for their purpose to appear?

Will the action, whatever it be that may be done by you, help it, hinder it, or be quite inoperative, and unsuited to the end? Who can tell? The vitality and activity of the organ are pioneering on the outskirts of the origin of Life, where their very existence is always at stake, and where the requisites are quite unfamiliar to you and me.

Clearly every single dose of medicine, every act done to influence such occult conditions, is an experiment. The social instinct is very hypersensitive to the use of this word in relation to disease; but why? We all when ill become the prey of experiment. Even the man who does nothing is not exempt. I suppose that there has never been a dose of medicine administered that has not been in some measure an experiment. "It subdues pain," you say. Yes. "It strengthens the heart." May be. "It arrests disease." No doubt. But how and why? They are the questions. And if it does these things for one of us, why does it fail for others? Further, what else is it doing in the economy at large—is its mission ended when your behests are accomplished? What about thyroid extract, Coley's fluid, atoxyl, and all the vaccines, serums, and so on, that are now coming into use? All powerful, even heroic, attempts to cure disease, what are they all in their use but experiments? And although the wind be tempered, as far as possible, to the shorn lamb—it is here that knowledge and experience come in—they are, and must be, experiments attended with risk. There are some who think and speak of experiments as if they were performed only upon the lower animals and the poor who cannot help themselves. The fact is that by experiments the worlds have grown; that experimentation is the one fundamental necessity of all progress, and the whole of animal life—life of every kind—to reap the benefit must share the risks and chance the pain. So each and all of us *must think* on these things, and try to give such individual thought and observation as is possible to the how and why of ordinary physiological reactions. We depend upon leechdom too much, and spend our time in thinking our leeches fools on the one hand or an all-knowing Providence on the other, and seem to be prone to handle the wrong end of the stick when it comes to a pinch. But it is not wise to leave these matters entirely to the republic of letters. Let us learn to steer our own craft amid the smoother eddies and currents of life's stream, so that when the call to the pilot comes we may be able, as we shall, to help him to apply his principles, derived from wider sources and more tutored insight, to the special needs of our immediate stress. As pilots we may also take heed of this.

And then what of the prospect?

With problems such as these before an expanding science, it cannot be one of royal pageant, or of easy achievement, and it is certain that the future triumphs over disease will only unfold themselves to an ever widening horizon that embraces all the latest advances of physical science as they bear upon the function of living. Yet would the possibilities seem to be limitless. With all the added information of recent years, the suggestions that have come to us from all directions by way of electricity and light and the ether that surrounds us, with all the forces that make towards us and for us, merging all Nature into one ordered whole, who can do other than put to himself this question:

"Can it indeed be that we are come to look unto the rock whence we were hewn?" Yet, with spectroscopic insight, with darkness dawning into light, with mental flashes borne across the storm, with all these new aspects of Nature surging on us, who shall say that we are now familiar with all forms of living energy—that there are no other sources still to be made known to us? In the sigh of the wind, the spirit of a voice, the magnetism of a presence or a touch, or that ineffable something in face or form that the painter tries to catch, is there nothing travelling that other way?

I trust the larger hope.

But to inquire into final causes, it has been said, inasmuch as they are beyond the grasp of the human intellect, is not the pursuit of a scientific mind. Such an opinion, since it would seem to ignore the stimulus, the insight, the instigations of the imagination, must be belated, if ever it could have been true. Still, reverencing as I do that attitude of mind thus expressed not long ago by Mr. Arthur Balfour, that to make the best of the future one must never ignore the past, you may, perhaps, think that, could Harvey have returned to us for this afternoon, he *might* have looked askance at the future, to which I have committed the influence of his immortality and the trend of the set purpose of his life. But methinks *he* would have absolved me, for where is that knowledgeful man—let him step forward that we may look at him—who has deliberately foresworn that pleasure—I will venture even to say profit—of a mind set free to roam awhile, who does not dream even while he works, whose mood is not rather:

And I who with expectant eyes
Have fared across the star-lit foam,
See through my dreams a new sun rise,
To conquer unachieved skies,
And bring the dreamer home.

It is only thus that Past, Present, and Future hold in one; why the Harveian Orator at this annual festival is bidden by our Master's expressed will to commemorate the Past. And the Fellows will count it indeed happy and appropriate that on such a day, by the filial and devoted generosity of its President, the College should receive its latest and priceless benefaction of the eleven recently discovered autograph letters, the very *ipsissima scripta* of William Harvey. Alive as these are with human interest, how the shade of Harvey seems to become re-embodied, and the passage of time to have been for the moment stayed!

The Past! I sometimes think, Sir, that the mundane casket of our Harveian librarian must be as full of ghosts as a queen bee is of embryonic honey. These walls are alive with memories of benefactors of the college. The dead herein still speak to us, and here is harboured the tomb of many a brilliant but buried thought awaiting its emancipation that still tarries. And with Pavy, Hughlings-Jackson, Wilks, Allchin, and others dear to us all gathered to the fathers of medicine almost since St. Luke's day last, what an addition to the supporters of the arms of this our ancient college within the bygone year. And of the *less* recent past, who is there entering this library and making his obeisance to your presidential chair that does not experience the calm and the charm of a great contentment when he remembers that the afterglow of Harvey and his followers still guides *him* on to that yet more perfect day of ever-increasing knowledge and less disputable truth?

And though long sunk from sight, I know,
The glory of your afterglow
Will never wholly fade.

PELLAGRA IN THE BRITISH ISLANDS.

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Of all known diseases pellagra is certainly the one which escapes recognition most easily, not that it lacks individuality, because few diseases are better characterized in their full manifestation; not that it is of little importance, because few diseases are more harmful and deadly, but because it varies greatly in the nature, sequence and intensity of its symptoms; because it may remain clinically latent for years in the patient and epidemically dormant for long periods in a given region; because it avoids crowded cities and, as a rule, strikes the uncared-for peasant in remote country districts; because it principally affects the young with manifestations usually so mild that it is either overlooked or unheeded; because it breaks out in the mature and elderly under the stress of lowered resistance following in the wake of poverty and famine and engrafting itself on to other diseases; and because, in most countries, it is concealed with shame as the brand of destitution and madness.

Ignorance and superstition in the patient, lack of knowledge and indifference in the physician, have not only added to the burden of the unfortunate pellagrin, but also greatly retarded the recognition, elucidation, and eradication of this grave and insidious disease, a prolific, though frequently unrecognized, cause of insanity.

It is no easy matter to trace far back the history of indubitable pellagra, because, in the past, it was confounded with many other diseases, such as scurvy, eczema, leprosy, syphilis, erysipelas, ankylostomiasis, etc.

Don Gaspar Casal, the learned and able physician of Philip V of Spain, as early as 1735, when practising in Oviedo, was the first to grasp the individuality and significance of this triform, protean disease, which he portrayed most skilfully in his *Memorias de Historia Natural y Medica de Asturias*.

In Italy the disease was first recognized in Feltre, about 1740, by Professor Giuseppe Antonio Pujati, and soon after, in Friul, by Drs. Nascimbeni and Anton-Gaetano Pujati, but nothing was published until 1771, when Francesco Frapolli observed it in the country round Milan and described it under the popular name of "pelagra," by which it had long been known to the peasantry in Lombardy. The recognition of pellagra in Italy towards the middle of the eighteenth century does not imply, as the majority of Italian physicians have hitherto held, that the disease had been imported or had arisen *de novo* at that time, because not only did Frapolli himself say it was "as old as the sun," but we have ample evidence to show that, at the time of its discovery, it was already widely distributed throughout North and Central Italy, and occupied, amongst the foot-hills of the Alps and Apennines, those very same localities in which it is now stationed. This, of course, means that it must have existed for centuries previously, because, like malaria, pellagra has never shown any tendency to rapid spread from place to place, but only fluctuations of prevalence and virulence within its endemic areas, which areas are everywhere associated with swift-flowing streams, just like malarious localities are associated with stagnant waters.

In France the disease was first recognized by Jean Hameau, a precursor of Pasteur, and famous for his work on *Les Virus*. Hameau began to observe pellagra in 1818, at La Teste de Buch, but the disease had long been known to the shepherds of the Landes, who had popular names for the disease and went to implore the saints at special shrines for deliverance—as at Bascons, for instance, where they got some ointment from off the transfixed hands of a local crucifix and besmeared with it their own discoloured and burning extremities.

In the Landes of Gascony pellagra used to be very prevalent about the banks of the Leyre and Ciron rivers and along the streams running from the central plateau to the sea coast. Since about 1890, owing to great engineering, agricultural, and piscicultural changes, both pellagra and malaria have greatly diminished, but pellagra has not entirely disappeared, as Régis and Marie had led us to believe. Cases still continue to crop up in the Landes and in the river valleys of the Pyrenean region, as we were able to ascertain during our recent investigations in the south-west of France.

The history of the discovery of pellagra in other countries—such as Hungary, Roumania, Egypt, etc.—shows everywhere, again and again, the same late and fortuitous recognition, notwithstanding the antiquity and wide distribution of the disease. Indeed, in many cases, as, for example, in Transylvania, Egypt, and Algeria, even after its presence had been unmistakably pointed out by competent men, it was long before it became generally acknowledged. Indeed, Hirsch, in his *Handbook of Geographical and Historical Pathology*, goes so far as to say that "The statements as to the occurrence of pellagra in Hungary and the Banat, in Egypt, and in Algiers, are based upon quite untrustworthy information."

Another curious feature in the history of pellagra is the fact that in countries where it has been known to prevail endemically for over a century, as in Spain, Italy, and France, we observe from time to time a peculiar tendency to consider pellagra a thing of the past.

No instance of oversight could be more instructive than that of the recent recognition of pellagra in the United States of America. Until five or six years ago the disease was looked upon as non-existent in North America, and for this reason was not even mentioned in medical textbooks. Yet it existed, and Grey and Tyler had both in 1864 recorded one case each. Later, a perusal of the old case-books of lunatic asylums brought to light other earlier cases, such as those of Trezevant in 1834. But it was not until 1907, when Drs. J. W. Babcock and J. J. Watson raised the alarm, that pellagra became a subject of inquiry in the United States. Now we know that the disease prevails in no less than thirty-three States, and that there are over 10,000 indubitable cases on record. Recent importation and spontaneous origin are quite out of the question. Only centuries of the prevalence of the disease can explain the wide range of pellagra in North America, extending from the Atlantic to the Pacific Ocean and from the Gulf Coast to the Great Lakes, especially when we consider the peculiar topographic distribution of the disease, which, in America as in every other affected country, is limited to the foreland or "Piedmont Section" of mountain ranges, and more particularly to the valley tracts of swift-flowing streams.

Although so far the British Islands have not been looked upon as a likely field for pellagra, we have no hesitation in stating that the disease is endemic, and has long been endemic in various parts of the United Kingdom. Two certain and quite typical cases have already been described, one by Dr. James C. Howden, in 1866, the other by Drs. R. Dods Brown and R. Cranston Low in 1909. Then we have earlier though vague information which acquires strength in the light of these cases, and, lastly, the result of our own recent investigations in the eastern districts of Scotland between the Firth of Forth and Pentland Firth, including the knowledge of two other grave and typical cases as yet unrecorded, one studied by Dr. R. Cranston Low, the other under the care of Dr. Box, subdean of St. Thomas's Hospital, London.

So long as pellagra was looked upon as a kind of poisoning due to the eating of damaged maize, or to some form of cachexia peculiar to the insane, the occurrence of one or two isolated cases of the disease could have relatively small importance, but since we have shown that pellagra is an infectious insect-borne disease the recognition of a few cases in separate places and at various times acquires a very different significance. It indicates the overlooking of a widespread and long-standing endemic disease of very grave nature.

So far as we are aware, the first notice regarding the presence of pellagra in the British Islands is the one given by Dr. E. Billod.[1]

Billod states that on December 14th, 1860, he received a letter from Dr. Brown, Inspector-General of the Scottish Lunatic Asylums, informing him that during an inspection in the most remote eastern parts of the island, in two idiots of the same family, he had observed an alteration of the skin which he qualifies as a kind of pellagra, adding that he had never seen any other cases of Italian pellagra, and regretting that he was unable to furnish more precise details. "I have considered it right," says Billod, "to place these cases on record, with the reserve, however, that they probably pertain to the pellagra which accompanies insanity." Believing pellagra to be a kind of cachexia due to poverty, and giving rise to an erythema under the action of the solar rays, he surmised that this pellagrous cachexia must perforce occur in Great Britain and Ireland, "where poverty is great beyond conception" (*sic*), without, however, being accompanied by any erythema, the sun in these parts not being powerful enough to occasion it. Thus one would have a *pellagra sine pellagra* such as is observed at times within the endemic centres of the disease, and analogous to *variola sine variolis*.

Very soon after the publication of Dr. Billod's work, Dr. James C. Howden, Medical Superintendent of the Montrose Royal Asylum, published[2] a case of typical pellagra which occurred in a woman aged 33 years, a factory spinner from Arbroath, Forfar, who was admitted into the asylum August 19th, 1863.

The next supposed case on record is one from Cheshire, England, published by Dr. Andrew Cassels Brown.[3] We prefer to dismiss it, however, as doubtful. The last case published is the one described by Drs. R. Dods Brown and R. Cranston Low.[4] It occurred in an unmarried woman aged 21 years, who was admitted to the Royal Edinburgh Asylum on July 13th, 1908, under the care of Dr. George M. Robertson. She was born in Shetland, and had lived there

all her life. The dermatitis in this case affected the face and the back of the hands, extending over the wrists and lower parts of forearms. The patient was delusional, complained of severe pain in the lower part of the back, presented spasticity of both legs, severe stomatitis, and diarrhoea.

In a paper read at the National Conference on Pellagra, in Columbia, South Carolina, U.S.A., in 1909, Dr. Sandwith stated that two cases had recently been recorded in patients who had never been out of the British Isles, but that the published accounts did not tally with the disease as he knew it. While agreeing with Dr. Sandwith with regard to Dr. Cassels Brown's case, we cannot agree with him with regard to Drs. Dods Brown and Low's case, which we consider to be a typical case of pellagra.

At the time of the International Dermatological Congress in Rome in April, 1912, we had the opportunity of taking Sir Malcolm Morris, Dr. Ian MacLeod, and Dr. R. Cranston Low to Zagarolo, a small village at the foot of the Prenestini Mountains, and showing them a number of pellagrin peasants encamped close to a typical *Simulium* stream. Dr. Low remarked that these cases and others from Northern Italy shown at the congress meetings exhibited symptoms identical with those presented by his Shetland case, and by another as yet unrecorded case from Fifeshire, which he had had the opportunity of examining in Edinburgh in 1911.

On our return to England this autumn we resolved to visit the eastern districts of Scotland, and possibly the Shetland Islands, and endeavour to ascertain all we could concerning the distribution and prevalence of pellagra in these parts. Owing to lateness of season, unfavourable meteorological conditions, and limited available time, we were unable to make as thorough and extensive a survey as we should have liked, but we were able to establish that pellagra is certainly endemic in Fifeshire, Forfarshire, Aberdeenshire, and the Shetland Islands.

Dr. Low having given us the name of the village in which his recent unpublished case had occurred, we visited it twice, and during one of these visits Dr. Low very kindly joined us. The village is Colingsburgh, halfway between Largo and Anstruther, on the eastern coast of Fifeshire. Here Mrs. R., the patient, was born and lived all her life. From time to time since the age of 8 she went for short visits to her relatives at Rothesay in Bute. She was born at a farm called Kilconquhar Mill, which is on the road between Colinsburgh and Kilconquhar, and which is situate on a slight elevation above a little stream or burn, in which we found many larvae and some pupae of Simuliidae. In the fields adjoining this stream we obtained very clear statements from the field labourers of the attacks of small biting flies coming in clouds in the early morning, and especially in the evening. Mrs. R. was about 40 years of age when she died of pellagra in 1911. The full clinical history of her case will be published in detail shortly by Dr. Low, and therefore we will restrict ourselves to the points which more directly concern us. Our knowledge is derived from Dr. Low, Dr. Bryson, the local physician under whose care the patient was, and the relatives of Mrs. R.

Mrs. R. suffered from an eruption on her face, which recurred in spring and autumn. The actual date of the commencement of this eruption was forgotten, but it was remembered that it was thought to be due to vaccination, and, if this association is correct, it probably started at a very early period of her life, a fact fully in accordance with our experience of Continental pellagra.

The eruption is stated to have been on the exposed parts of the face and arms. She is also said to have suffered from sore lips and increased amount of salivation and from diarrhoea on and off from girlhood. No history of having eaten maize could be obtained, but we were told that the patient had a peculiar habit of eating raw oatmeal, but when she acquired this habit no one remembers; apparently it was in early life. We could find no evidence of maize being used as human food in Colingsburgh, though apparently it is given to pigs. We were told that maize is often used along with other materials in the preparation of Scotch whisky, but this need not be seriously considered in this case. As regards social position, Mrs. R. appears to have been very comfortably provided for all her life. She had three children, the youngest being born in 1909. None of these children showed any signs of pellagra when inspected by us.

Dr. Bryson kindly informed us that Mrs. R. was vaguely ill after the birth of the baby in 1909, and became acutely ill in April, 1911, with a bright red erythema on the dorsum of both hands. At the time he saw her the eruption only extended to the wrist. There was no evident eruption on the face at that time, but there was an eruption on the nates, about the size of half the palm of the hand, on either side. A peculiarity which struck him was that the eruption on the left hand showed a clear band of skin corresponding to the area covered by the wedding ring and therefore not exposed to light. There was no vaginal discharge and there was diarrhoea, especially in the morning. She was inclined to turn faint and giddy and to fall down and she became very weak. In May, 1911, the mental symptoms became pronounced and she became apathetic and lost interest in everything, while she began to look foolish, and later she was sent to Edinburgh.

Her symptoms in Edinburgh were kindly described to us by Dr. Low, who also showed us a photograph and three admirable casts he had taken of her face and arms, but, as he will eventually publish in detail this portion of her history, we refrain from further remark beyond stating that we are entirely in accord with Dr. Low that Mrs. R. was a typical pellagrin.

In Colingsburgh we had the opportunity of examining Mrs. R.'s sister, and we are inclined to believe that she is also a pellagrin. She appears to be subject to a recurring eruption, not unlike a sunburn, which affects her face and the back of her hands in spring time, and this occurs with a more permanent eczematoid condition affecting the palms of the hands, as in the case of her deceased sister. However, she has been out of the United Kingdom.

On visiting the farm where Mrs. R. was born we endeavoured to ascertain whether we could trace any pellagra in the present occupiers; and, indeed, we did see a woman and a child presenting suspicious skin lesions on hands, arms, and face, but these people were unwilling to give us any information. In the fields close by we met a woman with erythematous eruption on the face, and a history of eruption on the dorsal surface of both hands and forearms, and this case we also regard with suspicion, even after considering the fact that she suffers from seborrhoea.

In the Royal Montrose Asylum we were shown a Shetland Islander from Uist, who was admitted to the Edinburgh Asylum on June 2nd, 1880, and thence transferred to the Montrose Asylum on June 30th, 1881, where he has been ever since.

He is suffering from chronic dementia, is not epileptic, and is said to have been suicidal before admission. The history is not very good; we are told that on admission he was unable to answer questions coherently, that he was restless, excited, and dirty in his habits. He was often wild, and has assaulted people. His left pupil was larger than the right one for some time. There is no doubt that from time to time he has suffered from diarrhoea, and that he constantly spits about owing to excess of salivation. The ward attendant told us that his hands have presented an eruption from time to time, especially about spring time, but this is not recorded in the history. The epidermis on dorsum of hands is in parts thickened, in parts atrophic. The feet and face are normal.

At the Aberdeen Royal Asylum the superintendent told us of a possible case from New Aberdour, Aberdeenshire, which had recently died in the asylum. The patient had an erythematous condition of the face and hands, and his mental state was that of chronic dementia. His skin lesions were recognized by the superintendent and the senior assistant medical officer as similar to those shown in our photographs of Italian pellagrins.

On the Ugie, a typical *Simulium* stream, near Peterhead, Aberdeenshire, we met several children presenting the peculiar skin lesions characteristic of pellagrin children after the subsidence of the eruptive stage.

Finally, on our return to London, Dr. Sandwith kindly informed us of the presence of a suspicious case at St. Thomas's Hospital, under the care of Dr. Box, and, thanks to the courtesy of the latter we were able to see a most severe and typical case in a boy who had never been out of England. The following is a note by Dr. Box:

"Recently there has been in St. Thomas's Hospital under my care an English boy suffering from the nervous and cutaneous manifestations of pellagra.

"The patient, who was 8 years old, was born at Leyton in Essex, moved thence at the age of 5 to Catford, and later to Slough. His illness first attracted attention in June of this year owing to an epileptiform attack. He died in hospital on October 18th.

"An extremely interesting fact is that this boy's brother was also under my care in 1910 with symptoms pointing to combined sclerosis of the spinal cord. His malady was not then recognized as pellagra. It was of longer duration than was the case with the younger brother, commencing soon after the age of 4 with difficulty in walking; the boy, however, did not come under observation until he was 11. The mother

states that his skin eruption was in every respect similar to that of the younger brother, and showed, as did the other, marked seasonal variations. The elder boy died early in 1911, and no *post-mortem* examination was obtained. It is hoped that a complete report of the first-mentioned case, together with the pathological findings, may be published in due course."

We believe that the information so far collected is sufficient to prove that for an undetermined but long time pellagra has endemically prevailed in parts of the United Kingdom, and that, notwithstanding the recognition and publication of two grave typical cases, the disease has been generally overlooked in its milder and more obscure forms.

The presence of pellagra in the British Islands is of geographic interest, because hitherto the disease was believed to be limited northwards by the forty-fifth parallel. Etiologically, its presence in the United Kingdom is of importance, because here its causation cannot be reasonably explained by the consumption of either sound or bad maize, and its topographic distribution shows that here, as elsewhere, it is linked to the swift-flowing, Simulium-infected streams. Sociologically, the disease calls for the most urgent attention. Pellagra is an insidious disease, either rapidly fatal or of long intermittent course, leading to insanity. The pellagrous psychical disturbances are as many-sided and as obscure as the somatic manifestations of the disease, but the salient feature is an intermittent and progressive amentia, often assuming a semblance of melancholia and exhibiting from time to time regular outbursts of maniacal excitement. In some cases there may be all the appearances of progressive paralysis. Dementia is the invariable termination, unless the patient be cured, or carried off sooner by some intercurrent disease or mere exhaustion of the vital powers.

A careful reading of the old case-records of our lunatic asylums will no doubt bring to light many cases of overlooked pellagra; but—and this is far more important—we trust that henceforth physicians will be on the look-out for this disease, and that they will make a thorough search for the milder forms which, as our experience shows, very easily escape detection.

However dormant it may be at the present time, a fatal, insanity-causing disease, such as pellagra is, cannot be allowed to continue unchecked in its progress, and it behoves us to take the matter very seriously in hand.

REFERENCES.

[1] *Traité de la pellagre*, 1865. [2] *Journal of Mental Science*, April, 1866. [3] *Practitioner*, May, 1906. [4] *Edinburgh Medical Journal*, September, 1909.

MITRAL STENOSIS: PERIPHERAL EMBOLI CAUSING PARTIAL MONOPLEGIA WITH SYMPTOMS SIMULATING CEREBRAL EMBOLISM.

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THIS case is of interest because on both occasions on which embolism occurred in a limb artery the attendant symptoms accurately simulated those associated with cerebral embolism. Thus on each occasion the patient was the subject of partial monoplegia, with attendant nausea and giddiness.

Additional interest is afforded by the fact that death was absolutely sudden, and was found to be due to the presence of a ball thrombus in the right auricle.

The patient, a married woman aged 42, had had mitral disease, following an attack of rheumatic fever six years previously.

The first embolus affected the left upper limb. It occurred eighteen days before admission to hospital. The patient while stooping suddenly felt sick and giddy, and the left forearm and hand fell powerless to the side. She retched but did not vomit, and she saw dark spots in front of the eyes. The second embolus, affecting the left lower limb, occurred three days later. "Without warning" she again felt sick and giddy, again saw dark spots before the eyes, and simultaneously "lost power" in the left leg and foot.

She was recommended as a case of cerebral embolism to University College Hospital, where she was admitted under the care of Sir John Rose Bradford, to whom I am indebted for permission to publish this case.

Considerable trophic changes in the affected limbs caused some doubt of this diagnosis, and the real state of affairs was revealed on noticing that the left radial pulse was absent.

Condition on Admission.

The facial muscles showed no paralysis or weakness. The skin of the left forearm and hand was cold, dry, and shining, and blebs containing serous fluid were present on the tip of each finger. Movements at the left shoulder and elbow were weaker than on the right. Pronation was only possible through an angle of 45°; supination was weak, but complete. Flexion and extension at the wrist and of the fingers were both markedly limited. No pulsation could be felt below the first part of the axillary artery. In the left lower limb also marked trophic changes were present, and pulsation was absent from some of the main arteries. The skin below the knee was white, shining, and oedematous. It was colder than on the right side. Over the front of the leg was an oval erythematous area, 8 in. long, in the centre of which was a smaller oval area, 3 in. in length, where the skin was blue. Over all this area the skin was hyperaesthetic. There was a small patch of anaesthesia on the dorsum of the left foot.

There was no paralysis on the left lower limb. Movements were weaker than on the right side, but the impairment was due rather to pain evoked than to any actual loss of power. Both knee-jerks were present; there was no ankle clonus, and the plantar responses were flexor. Sphincter control was normal. Examination of the heart showed well-marked mitral disease.

Death.

The patient died suddenly four days after admission. She was leaning over the bed, when she quickly raised her head and shoulders, became intensely cyanosed, made a few stertorous efforts at respiration and then stopped breathing. She had spoken to a nurse a few minutes before and had then appeared perfectly well.

Necropsy.

The *post-mortem* examination confirmed the opinions formed as to the nature and origin of the lesions in the upper and lower limbs and also supplied the explanation of the sudden death.

There were no cerebral lesions to account for the loss of power in the limbs.

As the right auricle was being cut open, there fell out an oval *ante-mortem* clot 2½ in. in its longer diameter. The endocardium of the walls and valves was smooth and free from adherent clot, so that the thrombus lay quite free within the auricle. The mitral orifice was buttonhole in shape and only admitted the tip of one finger. There were no vegetations to suggest malignant endocarditis.

The upper part of the left brachial artery was blocked by a reddish-grey clot evidently of recent formation. Another thrombus filled the lower half of the left femoral and the whole of the popliteal arteries below. This thrombus was white and organized. Hence, seeing that the leg lesion was the more recent, there must have been an earlier embolus in the femoral artery which had caused no symptoms, and either an extension of this or a second embolus must have been responsible for the symptoms and signs affecting the leg as described above. Whatever may be the correct explanation of the second series of symptoms, the additional interference with the blood supply brought about the subsequent commencing gangrene of the skin covering the tibia.

Thus the symptoms of nausea and giddiness accompanying a partial monoplegia were on both occasions misleading. No lesion to account for these could afterwards be found in the central nervous system. The loss of power in both instances was due to the local anaemia. The nausea and giddiness are more difficult to explain. They may only have been expressions of a general circulatory disturbance consequent upon the occurrence of a sudden peripheral embolism, or possibly they were due to reflexes originated from the walls of the plugged vessel. In this case the patient would be expected to have noticed some pain in the limb at the time of the embolism. Pain, however, was absent on both occasions. Hence the exact cause of the nausea and giddiness must be left open to conjecture.

ON THE PRESENCE OF SUGAR IN THE CEREBRO-SPINAL FLUID FROM CASES OF MENINGITIS.

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Whether the reducing substance be dextrose, saccharose, pyrocatechin, or a mucinoid matters not for the purpose of this short paper. There is in normal cerebro-spinal fluid a chemical substance which reduces Fehling's solution, and this is absent in some cases of meningitis and present in others. There appears in the literature of the subject considerable confusion as to the cases in which it is present or absent, and considerable uncertainty as to its value.

My own experience, based, it is true, on only a small number of cases, has led me to a definite conclusion on this subject, and I consider such examination to be of definite value.

The method adopted is to boil together equal portions of cerebro-spinal fluid and Fehling's solution in a narrow test tube and allow it to rest in a sloping position. In the course of an hour there will be a marked yellow deposit forming a band on the lower side of the tube, and with a little practice one can form a fairly accurate opinion as to whether such band is normal in amount or diminished; where sugar is absent no deposit will occur.

My results are as follows:

1. In 9 cases of tuberculous meningitis sugar was present in apparently normal amount in 1 case; was present in diminished amount in 7 cases; was present in markedly diminished amount in 1 case—this was taken only two hours before death.

2. In 7 cases of pneumococcus meningitis sugar was absent in every case.

3. In 3 cases of streptococcus meningitis sugar was absent in every case.

4. In 9 cases of cerebro-spinal meningitis sugar was absent in 2 cases and present in diminished amount in 7 cases.

5. In 7 cases of poliomyelitis sugar was normal in amount in 1 case and slightly diminished in 6 cases.

6. In 3 cases of mixed septic meningitis, secondary to middle-ear disease, sugar was absent in all 3 cases.

Andrew Connall, in a fine paper in the *Quarterly Journal of Medicine*, January, 1910, gives the results of the examination of the cerebro-spinal fluid for sugar in 133 cases of cerebro-spinal meningitis, testing them in every stage of the disease. He comes to these conclusions:

> The results prove that sugar is absent from the fluid during the acute stages of cerebro-spinal fever. This finding was invariable, and it depended, as previously shown, on the presence of active acid-forming meningococci. . . . Sugar, however, is not absent at all stages of cerebro-spinal fever, and during the chronic periods of the disease Fehling's solution may be reduced in the presence of living organisms. It may be said, however, with certain reservations, that the reappearance of the sugar is an indication that the disease is in process of cure.

He also examined 122 specimens from 69 cases of tuberculous meningitis. He states his results thus:

> In tuberculous meningitis sugar was found to be present in the great majority of the cases, and at all stages of the disease; 122 specimens were examined. An active reduction on boiling of the Fehling's solution occurred in 15, a partial but decided reduction in 102, and no reduction at all in only 4 instances. Complete absence of sugar was observed in only 2 cases. It was noted in the last week of illness, three times in one case and once in the other, and in each case at other times there was a slight reduction of Fehling's solution.

I would state the conclusions thus:

1. In pyogenic meningitis, pneumococcus, streptococcus, and mixed infection sugar is invariably absent.

2. In cerebro-spinal meningitis sugar is absent in the acute stage, but may return in some degree as the infection recedes.

3. In tuberculous meningitis sugar is present, except in very rare cases shortly before death, in which stage difficulty of diagnosis rarely exists.

4. In poliomyelitis sugar is present.

These findings seem sufficiently definite to be of real value.

The Science Committee of the British Medical Association,

THE ANAEMIA OF ANKYLOSTOMIASIS.

By WILLIAM NICOLL, M.D., D.Sc.,

Ernest Hart Scholar.

The Blood Volume in Hookworm Disease (Ankylostomiasis).

In regard to the first subject of research the conclusions I venture to draw from my experiments are:—

1. Hookworm disease in dogs is not analogous to hookworm disease in man. In the latter the disease is essentially chronic and progressive, gradually leading to a fatal termination. No great degree of resistance appears to be acquired, although some individuals are apparently less susceptible to infection than others. Again, individuals of all ages are equally liable to acquire infection. In the dog, however, the disease is very acute and rapidly fatal. While all dogs appear to be liable to a greater or less degree of infection, only young dogs (under six months) succumb to the disease. In older dogs infection cannot be pushed beyond a certain point, even with long continued administration of infective material. In those a certain degree of anæmia is slowly produced, but it is also gradually recovered from, and the animal may regain its normal state, even although it remains infected with the worm, which infection, it may be added, may last over two years.

2. The anæmia in young dogs was characterised by great loss of weight, emaciation, prostration, intestinal hæmorrhage, greatly reduced temperature, and accelerated and irregular cardiac beat. These symptoms were present to a less extent in older dogs suffering from a minor degree of anæmia. In the latter the erythrocytes were rarely reduced in number to any great extent, but the hæmoglobin fell in some cases as low as 50 per cent. of normal. In the acute disease of young dogs, however, the erythrocytes were observed under one million per c.mm., and the hæmoglobin 10 per cent. In both cases the blood picture was characterised by the large number of erythroblasts, not infrequently of large size, and polychromatophilia. Eosinophilia was not a constant sign either of infection or of disease, and, in fact, was observed only on few occasions.

3. Infection is generally accompanied by fairly profuse intestinal hæmorrhage, which is most marked in the early stages, and tends to disappear later.

4. The blood volume of dogs suffering from the minor degree of hookworm anæmia is not materially altered, but, if anything, is somewhat diminished. The oxygen capacity of the blood per unit of body weight is also, on the average, somewhat decreased. In the acute stage of the disease the blood volume appears to be decidedly reduced.

5. In the early stages of infection the worms attach themselves to the lower reaches of the ileum, but in the course of time they are found further forward.

6. Cats are much less easily infected than dogs, while monkeys and man were found to be insusceptible to infection with the dog hookworm.

FLIES AND OTHER INSECTS AS CARRIERS OF INFECTION.

By WILLIAM NICOLL, M.D., D.Sc.,

Ernest Hart Scholar.

The Length of Life of the Rat Flea.

Rat fleas (*Ceratophyllus fasciatus*) were removed from their host and kept under various conditions of light, temperature, moisture, etc., in order to determine their length of life without feeding. Altogether sixty-four experiments were performed, involving a total of 463 fleas. The average length of life at ordinary temperatures was found to be 6·8 days, but the maximum in one experiment was over forty days. Two per cent. of the fleas lived for over three weeks, and 10 per cent. for over a fortnight. They live longer in winter than in summer, the difference being about two and a-half days on an average. At

very low temperatures (0°—8° C.) their life may be prolonged to over two months. Both excessive moisture and excessive dryness are inimical, but a certain amount of moisture is beneficial. Darkness does not prolong their life, for those which were kept in bright daylight lived, on an average, longer than those kept in the shade or in darkness.

It was further found that fleas may remain in the larval and pupal stages for much longer periods than normal under certain circumstances. Several experiments were made to determine this point. In the first, material containing fleas was removed from a rat cage and isolated. It was observed to remain infected with fleas for a period of over five months. A second experiment continued to produce fleas for a period of eleven months. In a third experiment this period was extended to fourteen months. Some fleas in this experiment at the end of a year were placed along with a young rat, and in three months they had multiplied very considerably.

From these experiments it is evident that the larval and pupal stages of the rat flea, which normally last from two to six weeks, may be prolonged to over a year. As seen in the first set of experiments, the length of life of an adult flea under these conditions is generally under a month, and no evidence was obtained that the fleas multiplied during the period, although this may be possible; so that it may be concluded that the appearance of adult fleas after such a long period as a year was due to the larval and pupal stages being prolonged.

The importance of these observations to preventive medicine lies in the fact that as fleas are carriers of disease, it may be—and is—of considerable moment to determine exactly how long they can exist under various conditions. A full account of these observations is being prepared for publication.

THE OPSONIC INDEX IN PLAGUE VACCINATION.

By RALPH St. JOHN BROOKS, M.B., D.P.H.,

RESEARCH SCHOLAR.

[ABSTRACT.]

THE production of immunity in rats vaccinated against plague, is correlated with the production of immune opsonin in the serum of such animals.

Various anti-bodies appear in the serum of animals inoculated with plague vaccines, but the development of agglutinins and bactericidal substances, for example, do not lend themselves to satisfactory quantitative study, in the case of pest. The opsonic response has been studied in man by Douglas,* who found a good response after inoculation with Haffkine's Prophylatic. It, therefore, seemed advisable to correlate the development of opsonin with the absolute immunity of some susceptible animal in order to see whether the amount of opsonin produced could be used as evidence of the development of immunity in man.

In most of the following experiments the results obtained with a single dose of vaccine are recorded in relation to the opsonic index; in others the results of repeated dosage are shown.

* Private communication.

The scope of the enquiry resolved itself into the following points:—

1. Is there any correlation between immunity and opsonic production in plague infection?

2. What is the influence of previous vaccination in the experimental animal?

3. What is the source of opsonin in immune plague serum?

METHODS OF EXPERIMENT.

In all cases the dilution method of Klien (1) was employed.

A strain of avirulent plague bacillus killed in 50 % formalin solution was employed in the opsonic technique.

The nucleo-protein used as a vaccine was prepared in the manner described by Rowland in the *Journal of Hygiene* (2).

The bacterial emulsions were prepared from 24 hour agar slopes.

The pooled serum of ten rats were used in each experiment.

EXPERIMENTS ON RATS.

The following is a summary of the results obtained on estimating the opsonic indices of the various pooled sera from day to day (see table at foot of page).

By plotting out curves based on these figures, it will be seen that those obtained with 1/10 mg. whole bacillus vaccine, and 1/100 mg. nucleo-protein are very similar, though in the former case the maximum is displaced somewhat to the right. The doses correspond, as it has been shown that the nucleo-protein contained in the bacillus is about 1/10 the weight of the whole organism. The smallest response is elicited with the smallest dose (1/12000 mg. nucleo-protein) and the largest response with the greatest dose (1 mg.) No response is elicited with the bodies of the bacilli washed free from nucleo-protein, and used as a vaccine, or with nucleo-protein solution which had been hydrolysing for several months, though the hydrolysing process has no effect on the immunising property of the nucleo-protein.† The general conclusions from these experiments would seem to show, as found by S. Rowland, by directly determining the protection aroused in rats to a subsequent fatal dose of living pest, that the soluble nucleo-protein contained in the plague bacillus is the responsible agent in the production of opsonin. The production of opsonin as first rises with the immunity. In the first two series on the table the maximum response is reached by the first day, by which time full immunity is established with this dose of vaccine.

HUMAN EXPERIMENTS.

The dose used in each case was 1/10 mg. of hydrolysed nucleo-protein, which is about the quantity of nucleo-protein contained in 1cc. of Haffkine's Prophylatic. The first experiment was conducted on myself (R. St. J. B.), the second on five persons who had been inoculated, like myself, some time previously with Haffkine's Prophylatic (P. I.), and the third on

† In the latter experiment the absence of opsonin is somewhat remarkable and may possibly be due to hydrolysis having gone too far, or to the carbolic added as a preservative interfering in some way with the opsonic response. It was subsequently shown that rats, inoculated with this particular vaccine, did not acquire immunity.

DOSE AND NATURE OF VACCINE.	HIGHEST DILUTION OF SERUM WHICH SHOWS A GREATER PHAGOCYTIC INDEX THAN SALINE CONTROL.										
	Before inoculation.	10 hours.	22 hours.	2 days.	3 days.	4 days.	5 days.	6 days.	7 days.	8 days.	9 days.	10 days.
1/100 mg. nucleo-protein ..	1/16	1/2048	1/4096	1/2048	—	1/1024	1/1024	—	1/256	on 18th	day.	
1/10 mg. whole bacillus ..	1/16	1/1024	1/4096	1/4096	1/2048	1/1024	—	1/1024	1/64	on 21st	day.	
1/12,000 mg. nucleo-protein	—	—	1/512	1/1024	1/2048	1/2048	1/1024	1/828	—	1/512	—	—
1 mg. nucleo-protein ..	—	1/4096	1/6044	1/4096	1/6044	1/2048	1/2048	—	—	1/1536	—	—
1/10 mg. whole bacillus (repeated dose after 35 days)	—	1/4096	1/16384	1/24476	1/24476	1/16384	—	—	1/4096	—	—	—
1/10 mg. nucleo-protein (hydrolysing for 8 months)			No response.									
9/10 mg. of washed bodies of bacilli			No response.									

five normal persons (P. II.) The following Table shows the highest serum dilutions which gave an index higher than normal saline.

	Highest dilution of Serum which shows a greater phagocytic index than normal saline.								
	Before Inoculation.	3 days.	4 days.	5 days.	6 days.	7 days.	8 days.	9 days.	10 days.
R. St. J. B.	—	1/1024	1/768	1/1024	1/1536	1/512	—	1/512	—
P. I.	—	1/512	1/512	1/1024	1/1024	1/512	1/512	1/256	—
P. II.	less than 1/32	1/128	1/128	1/256	1/384	1/256	—	—	1/192

Except in two cases in the third series, the effects, on inoculation, were very slight and in no case was any increase in pulse rate or temperature observed. In the third series the opsonic response is not as great as in series I. or II. In the case of the two groups of pooled sera the number of persons in each group is so small that one would hesitate to draw any definite conclusions from these results, but as far as they go, they tend to show that pre-existing sensibility has an influence on the opsonic index in human plague vaccination.

CONCLUSIONS.

1. The substance which produces a rise in the opsonic index in immune pest serum is the nucleo-protein contained in the bodies of the bacilli.
2. The washed bodies of the bacilli when used as a vaccine do not cause any increase in the opsonic index.
3. Within limits, the larger the dose the greater the opsonic response.
4. A repeated dose of vaccine raises the index above the maximum of the first inoculation, even after the curve has fallen to its normal level.
5. In the early stages, in rats, the opsonic response and the degree of protection aroused rise together.
6. In human beings the maximum response is observed to be much later than in the case of laboratory rats, and the response in a series of previously sensitised persons is somewhat greater than in a series of persons not previously vaccinated.
7. The local and constitutional effects of nucleo-protein vaccination compare very favourably with those observed in other methods of protective inoculation.

I desire to express my thanks to Dr. C. J. Martin, F.R.S., Director of the Lister Institute, and to Dr. J. C. G. Ledingham, Chief Bacteriologist, Lister Institute, for valuable advice and suggestions throughout the enquiry, and particularly to Dr. Sydney Rowland, of the Yersin Laboratory, Elstree, Herts, without whose constant advice and help the experiments could not have been carried out.

REFERENCES.

1. Klien: *Johns Hopkins Hosp. Bulletin, xviii.*, p. 245.
2. Rowland: *Journal of Hygiene, Vol. 10, No. 3*, p. 536.

THE DESTRUCTION OF ALKALOIDS BY EMULSIONS OF THE BODY TISSUES.

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DURING recent years many workers have investigated both the natural tolerance to drugs, that occurs in certain animals, and also the tolerance that is acquired when certain drugs are administered repeatedly. We now know that the protective mechanism, which causes tolerance, differs in the case of different drugs, and that, with the same drug, tolerance may be produced by different mechanisms in different animals. This evidence has been recently reviewed by Dixon (1); it appears that tolerance may be established by the following mechanisms: (1) Diminished absorption. (2) Increased rapidity of excretion. (3) Increased resistance of the tissues upon which the drug acts. (4) Linkage of the drug with other bodies to form an inert substance. (5) Absorption of the drug by organs upon which it has no action. (6) Destruction of the drug by the body tissues. All of these six mechanisms, except the third, have been shown to exist in mammals, the third has been demonstrated chiefly in protozoa and may depend upon physical causes. The sixth mechanism, the destruction of the drug by the body tissues, has been studied in various ways, both in the intact animal, in isolated surviving organs, and in emulsions made of excised organs; the present paper is concerned only with results obtained by the last method.

1. HISTORY.

Numerous investigators have examined the power of emulsions of the tissues to destroy alkaloids, and the method employed has usually been the following: An organ is excised from a freshly killed animal, and made into a fine emulsion with normal saline, a small quantity of alkaloid is added, and the mixture incubated for some time at body temperature, in the presence of oxygen; after incubation the alkaloid is extracted and estimated. Using this method Buys (2) showed that the livers of the frog and the rabbit could destroy hyoscyamine, but that the frog's blood, and the dog's liver did not do so. Cloetta (3 and 4) showed that the rabbit's liver could destroy atropine, he also considered that the rabbit's brain, and the cat's liver and brain could destroy atropine. Fleischmann (5 and 6), and Metzner (7) both showed that the rabbit's serum could destroy atropine, but that this power varied greatly according to the locality from which the rabbit came; they noted particularly that the serum of rabbits suffering from goitre did not destroy atropine. Fleischmann moreover showed that normal human serum did not destroy atropine, but that the serum of patients suffering from exophthalmic goitre could do so. Cloetta and Fischer (8) showed that neither the dog's liver, brain, nor heart could destroy digitoxin.

Several investigators have compared the action, which emulsions of the organs of normal animals have upon a drug, with the action of emulsions of the organs of animals made tolerant to the drug. Albanese (9) showed that liver emulsions from normal dogs did not destroy morphine, but that liver emulsions from dogs made tolerant to morphine could destroy considerable quantities of the drug. Dixon (1) showed that the liver of normal rabbits possessed a slight power of destroying nicotine, and that this power was increased in rabbits in which tolerance to the drug had been induced. Cloetta (4) compared the destruction of atropine, produced by the livers of normal cats and rabbits, with that produced by the livers of cats and rabbits made tolerant to atropine; he showed that the tolerant rabbits possessed increased powers of destruction, but that there was no such increase in the tolerant cats.

2. METHOD.

The method adopted by the writer was similar to that already outlined. A freshly exised organ was made into a fine emulsion by pounding in a mortar with sand and normal saline, and then filtering through muslin: a few drops of toluol were added to prevent sepsis, a minute quantity of the alkaloid to be tested was next added, and the mixture well shaken, and then incubated for a few hours at body temperature; after incubation the alkaloid was extracted by the Staas method, and the amount recovered estimated. It was found by experiment that heating to 60°C. completely removed the power of emulsions to destroy alkaloids; control experiments were therefore made by always taking two equal portions of emulsion, and heating one portion to 60°C. for 30 minutes before adding the alkaloid; the two portions were then treated in an exactly similar manner, and the amount of alkaloid recovered from each at the end was compared. In all cases small quantities of alkaloid (usually less than 1 mgm.) were added, and destruction of the alkaloid was only considered to have been proved, when, firstly the unheated portion was found to contain either no alkaloid, or at most a trace, and secondly there was a large difference between the amount recovered from the heated and the unheated portions; in this manner errors due to the extraction and estimation of such minute amounts of alkaloid were eliminated as far as possible.

3. MODE OF DESTRUCTION OF ATROPINE BY THE TISSUES.

The livers of frogs and rabbits were used, since emulsions of these organs were known to possess the power of destroying atropine (Buys (2)). The atropine was estimated by Vitali's test; the extracted alkaloid being diluted until Vitali's test was only just positive. When 1 c.c. of fluid was taken I found that 0.003 mgm. of sulphate gave a distinct violet colour, and that 0.002 mgm. gave a very feeble violet colour; smaller quantities showed a red colour, but no violet colour. The sensitiveness of the test depends upon the fuming nitric acid used, and upon the freshness of the alcoholic solution of caustic potash. Cloetta (4) found the end point with this method to lie between 0.005 and 0.01 mgm. of atropine, but the end point is so indistinct that no two observers are likely to obtain exactly similar results.

TABLE I.

POWER OF FROG'S TISSUES TO DESTROY ATROPINE.

1 or 2 c.c. of Emulsion of the organ mixed with Atropine Sulphate and incubated for some hours at 22° C.

Nos.	Date.	Organ.	Quantity of Emulsion in c.c.	Quantity of Atropine Sulphate added in mgm.	Length of Incubation in hours.	Quantity of Atropine Sulphate (in mgrm.) recovered.	
						i. From unheated Emulsion.	ii From Control. (Emulsion previously heated to 60° C.)
1	4.12.11	Liver	2	0·2	3	Nil	Atropine present
2	10.11.11	,,	2	0·4	2	,,	,,
3	14.11.11	,,	2	2·5	15	,,	,,
4	4.12.11	Heart	1	0·2	3	0·01	over 0·02
5	4.12.11	,,	1	0·1	3	nil	,,
6	8.1.12	,,	1	0·04	2	less than 0·002	0·04
7	21.11.11	Kidney	1	0·05	4	nil	Atropine present
8	18.1.12	,,	1	0·04	2	,,	,,
9	8.1.12	Skeletal muscle	2	0·02	2	0·01	,,
10	14.11.11	,,	2	0·025	19	over 0·01	,,
11	21.11.11	Pancreas	1	0·03	4	Atropine pres.	,,
12	4.12.11	,,	1	0·02	4	,,	,,
13	18.1.12	Blood	1	0·04	2	0·02	0·01
14	8.1.12	,,		0·02	3	Atropine pres.	Atropine present
15	18.1.12	Intestinal mucous membrane	1	0·04	2	,,	,,
16	4.12.11	Testicles	0	0·2	2	,,	,,

Cloetta (4) used this method and apparently considered it accurate within 10 per cent., as he draws conclusions upon differences of 10 or 20 per cent. I consider that the method cannot be relied upon to show a difference of less than 50 per cent. I found it possible to recover about 0.002 mgm. of atropine from 1 c.c. of tissue emulsion mixed with 0.01 mgm. of atropine. No atropine could be isolated when 0.005 mgm. of atropine was added to 1 c.c. of tissue emulsion. The unavoidable loss of atropine, therefore, is about 0.005 mgm. per c.c of emulsion used.

In some experiments the organ emulsion itself was tested, without extraction, by putting a drop of it into the eye of a kitten or rat. This test, when positive, was considered to prove the presence of atropine, but when negative the result was confirmed by extracting and testing by Vitali's test.

With a rat one drop of a solution of 0.01 mgm. atropine sulphate per c.c. always caused dilatation within forty-five minutes; a solution of 0.005 mgm. per c.c. did not always produce dilatation. With a kitten three instillations of one drop with intervals of ten minutes produced dilatation in sixty minutes with a dilution of 0.002 mgm. per c.c.; this corresponds to about 0.0004 mgm. actually introduced into the eye. Metzner (7) found that about 0.0001 mgm. of atropine sulphate put into a kitten's eye produced a well-marked dilatation. I made a few experiments with excised frogs eyes but found that they were much less susceptible; a concentration of 0.2 mgm. atropine sulphate per c.c. was required to produce dilatation.

(a) *The Rate at which Atropine is Destroyed.*

Expt. i.: Frog's liver emulsion 0.05 c.c. + 3.5 c.c. 0·65 NaCl. 1 c.c. of mixture put in four test tubes—1 drop of toluol in each tube. 0·04 mgm. of atropine sulphate added to each tube. Incubated at 22°C. for varying times, killed by heating to 100°C., atropine extracted and estimated by Vitali's test.

Test Tube.	Length of Incubation.	Atropine originally present.	Atropine recovered.
1	Killed at once	0·04 mgm.	0·03 mgm.
2	Killed after 5 min. incubation	0·04 mgm.	0·02 mgm.
3	,, ,, 30 ,, ,,	0·04 mgm.	0·008 mgm.
4	,, ,, 130 ,, ,,	0·04 mgm.	0·000 mgm.

This experiment shows that the destruction of atropine proceeds slowly, and that the action resembles that of a ferment rather than that of an ordinary chemical combination.

(b) *The Nature of the Atropine Destroying Substance.*

As already mentioned, the power of emulsions to destroy atropine was found to be completely abolished by heating to 60°C. The action was not however dependent upon the presence of living cells, for if an emulsion of liver was smeared upon glass plates and dried, the dried material when made into an emulsion with saline could destroy atropine and retained this power for several weeks; after keeping for a few months this power to destroy atropine disappeared. On filtering a liver emulsion through a Berkefeld filter the filtrate was found to possess the power of destroying atropine, but after filtering through a Pasteur-Chamberland filter the filtrate was inactive. The destruction of atropine is therefore due to a body which resembles a ferment in its general charateristics, it acts slowly, it is destroyed by heating, but it does not depend upon the presence of intact cells.

4. THE DISTRIBUTION OF THE ATROPINE DESTROYING FERMENT IN THE BODIES OF THE FROG AND THE RABBIT.

A series of experiments was made according to the method already described, using emulsions prepared from the different organs of the body. If, on prolonged incubation, an organ emulsion did not destroy nearly all of the minute quantity of atropine introduced, it was considered that the organ had no power to destroy atropine, and in these cases it was not considered necessary to make an exact determination of the atropine recovered.

From these results it appears that the frog's liver can descroy considerable quantities of atropine, the heart and kidney can destroy minute quantities, but the other organs possess no such power; it is especially noteworthy that the frog's blood possesses no power of atropine destruction, a conclusion also obtained by Buys (2)

The destruction of atropine by the frog's heart was further tested upon the surviving excised heart. The heart was perfused with a small quantity of fluid by means of a cannula introduced into the ventricle through the sinus venosus.

Expt. ii.: 7/2/12. 2 c.c. of saline in frog's heart, 0·04 mgm. atropine. Heart maintained a good beat for twenty-four hours. One drop of fluid tested on rat's eyes at intervals. Atropine disappeared after five hours.

After twenty-four hours fluid and heart extracted, and the extracts tested for atropine. No atropine found in either.

A control was made by leaving 0·04 mgm. atropine in contact with a dead heart under similar conditions. No destruction of atropine occurred.

From these experiments it appears that the rabbit's liver and serum can destroy atropine. This result confirms the experiments of Buys (2), Cloetta (3), Fleischmann (5 and 6), and Metzner (7).

The other organs of the rabbit appear to have no power to destroy minute doses of atropine, even on prolonged incubation. Cloetta (1 and 3) used a similar method, but obtained a different result. He took 10 gms. of the organ emulsion and added 5 mgm. of atropine sulphate, and incubated for thirty minutes. He found that the rabbit's brain, as well as the liver, possessed the power of destroying atropine. I have been unable to confirm this result. Cloetta also found that the muscles destroyed about 20 per cent. of the atropine introduced, but I consider that this difference lies within the limits of the experimental error of the method used.

TABLE II.

POWER OF RABBIT'S TISSUES TO DESTROY ATROPINE.

Emulsions incubated at 37° C.

Nos.	Date.	Organ.	Quantity of Emulsion in c.c.	Quantity of Atropine Sulphate added in mgm.	Length of Incubation in Hours.	Atropine Sulphate (in mgm.) recovered from	
						i. Unheated Emulsion.	ii. Control (Emulsion previously heated to 60° C.)
1	24.11.11	Liver	1	0·1	4	Nil	Atropine present
2	9.12.11	,,	1	0.5	5	,,	,, ,,
3	2.1.12	,,	2	0·1	15	,,	0.1
4	5.1.12	Serum	1	0·01	5	,,	0·01
5	27.11.11	Heart	2	0·4	15	Atropine present	Atropine present
6	16.11.11	,,	2	0·2	5	,, ,,	,, ,,
7	9.1.12	,,	2	0·04	5	0·02	0·02
8	16.11.11	Brain	1	0·1	15	Atropine present	Atropine present
9	9.1.12	,,	2	0·04	5	0·02	0·02
10	27.11.11	Intestinal mucous membrane	2	0·4	15	Atropine present	Atropine present
11	4.12.11	,,	1	0·2	5	,, ,,	,, ,,
12	27.11.11	Stomach mucous membrane	2	0·4	15	,, ,,	,, ,,
13	8.12.11	,,	1	0·2	4	,, ,,	,, ,,
14	9.1.12	,,	2	0·04	5	0·02	0·02
15	27.11.11	Pancreas	1	0·2	15	Atropine present	Atropine present
16	27.11.11	Kidney	2	0·4	15	,, ,,	,, ,,
17	27.11.11	Bone marrow	1	0·2	15	,, ,,	,, ,,

5. THE POWER OF THE TISSUES OF OTHER ANIMALS TO DESTROY ATROPINE.

In the rat, the liver, serum, heart, pancreas, kidney and intestinal mucous membrane were all tested, quantities of atropine varying from 0·2 mgrm. to 0·02 mgrm. were found not to be destroyed after incubation for four hours, there is therefore no evidence that any of the rat's organs can destroy atropine.

In the cat similar experiments were performed with the liver, heart, brain, pancreas, kidney and intestinal mucous membrane, in no case was there any evidence that atropine was destroyed, even after incubation for sixteen hours.

Cloetta ([3] and [4]) using a similar method, but using larger quantities of atropine (4 to 5 mgrm.) concluded that the cat's liver destroyed 50 per cent. and the brain 25 per cent. of the atropine introduced, as already mentioned, I consider that the method he used was unsuitable for estimating accurately the total quantity of atropine recovered. If the cat's tissues possess any power to destroy atropine, minute quantities of atropine should be almost completely destroyed after prolonged incubation; as I have shown, such a destruction occurs regularly with the rabbit's liver, but is never observed with any of the cat's tissues. A few experiments were made with the dog's liver, this was found to have no power to destroy atropine, a result previously obtained by Buys ([2]).

6. THE DESTRUCTION OF PHYSOSTIGMINE BY THE TISSUES.

The drug was extracted in the same manner as atropine, and the extracted drug estimated by dropping into the eye of a kitten. When three drops of a solution of physostigmine were put into the kitten's eye at intervals of ten minutes, it was found that a concentration of 0·02 mgm. per c c. produced contraction within one hour, but that a concentration of 001 mgm. per c.c. produced no contraction. Experiments were made with the frog's liver and the rabbit's liver, in neither case was there any evidence that physostigmine was destroyed.

7. DISCUSSION.

It is interesting to consider what relation there is between the power of the tissues of an animal to destroy atropine, and the degree of tolerance shown by the animal to this drug. I have shown that the frog's liver, the rabbit's liver, and the rabbit's blood can destroy atropine, but that there is no evidence that any of the tissues of the cat, dog or rat possess this power.

Unfortunately it is difficult to determine the relation between the power of an animal to destroy atropine, and its tolerance to the drug, as measured by the minimal lethal dose per kilo of body weight, because there is no general agreement as to the latter. Different observers give widely different numbers for the minimal lethal dose of atropine both for the rabbit and dog. This diversity of opinion may be explained by the fact noted by Fleischmann, that rabbits from different localities show different powers of resistance to atropine. I determined the minimal lethal dose of atropine administered subcutaneously for rats, but found that it varied greatly, being much lower in young rats: 0·5 grms. atropine per kilo body weight killed young white rats weighing about 50 grms., but over 2 grms. atropine per kilo were required to kill older rats weighing 200 grms. Cloetta ([4]) compared the minimal lethal subcutaneous dose of atropine in cats and rabbits and found it to be 0·03 grms. per kilo and 0·5 grms. per kilo respectively: thus showing that the rabbit whose liver can destroy atropine is much more tolerant than the cat whose liver possesses little or no such power.

Heffter ([10]) compared the minimal lethal dose of atropine in rabbits and dogs, and obtained the following figures:—

TABLE III.

Animal.	Mode of Administration.	Dose of atropine Sulphate in grm. per kilo.
1. Rabbit ..	By mouth	1·4-1·5
2. ,, ..	Subcutaneous	0·65-0·7
3. ,, ..	Intravenous	0·068-0·074
4. Dog	Subcutaneous	0·401
5. ,, ...	Intravenous	0.07.

The rabbit therefore shows greater power of resistance to the drug administered subcutaneously, but both animals are equally susceptible to the drug when it is given intravenously; as already mentioned the rabbit's liver can destroy atropine but the dog's liver cannot do so. Cloetta also compared the action of atropine upon normal rabbits, and upon rabbits in which tolerance to the drug had been induced. The tolerant animals were more resistant to atropine given subcutaneously, but when atropine was given intravenously, and the minimal dose ascertained which would paralyse the vagi, it was found that this dose was the same both in the normal and in the tolerant animals.

In the cases recorded therefore it appears that there is some relation between the power of an animal to destroy atropine and its tolerance of the drug, when measured by the subcutaneous minimal lethal dose; this is shown by the following table:—

TABLE IV.

No.	Animal.	Subcutaneous Minimal Lethal Dose of Atropine in grms. per kilo body weight.	Authority.	Power of Liver to Destroy Atropine.
1	Frog	3·0	Determined by the writer	Well marked
2	Rabbit	0·7	Heffter ([10])	Well marked
3	Rat	2·0	Determined by the writer	Nil
4	Cat	0·03	Cloetta ([3])	Nil
5	Dog	0·4	Heffter ([10])	Nil

This comparison is not satisfactory because of the uncertainty as to the true minimal lethal dose in any species. It will b seen that some of the more tolerant animals possess the power of destroying atropine, but that the rat is exceptional since it is very tolerant to atropine, but none of its tissues have any power to destroy the drug.

Both Cloetta and Heffter show that an animal may be tolerant to subcutaneous doses but show no tolerance to intravenous doses, this is only natural because, when given intravenously, the drug exerts its specific action before sufficient time has elapsed for any appreciable destruction to occur. This rule probably holds good for most other drugs in which tolerance is observed, and with the development of intravenous therapy it may become of considerable practical importance.

CONCLUSIONS.

1. The livers of the frog and rabbit possess the power of destroying atropine; this power persists when all living cells are destroyed, and is due to a soluble body, resembling a ferment in its action.
2. In the frog the liver has a marked power, and the heart and kidneys have a slight power, to destroy atropine; no other tissues have any such power.
3. In the rabbit the liver has a marked power, and the blood a less marked power, to destroy atropine; no other tissues have any such power.
4. None of the tissues investigated in the cat, the rat, and the dog have any power to destroy atropine.

REFERENCES.

1 Dixon.—Proc. Roy. Soc. Med., 1911, Vol. 5, Therap. Sect., p. 1.

2 Buys.—Annales de la Soc. Roy. de Scien. Med. et Nat. de Brux., T.IV., p. 73, 1895.

3 Cloetta.—Arch. f. exp. Path. u. Pharm. Suppl., 1908, S. 119.

4 Cloetta.—Arch. f. exp. Path. u. Pharm., B. 64, S. 427, 1911.

5 Fleischmann.—Arch. f. exp. Path. u. Pharm., B. 62, S. 518, 1910.

6 Fleischmann.—Zeitschr. f. Klin. Med., B. 73, S. 174, 1911.

7 Metzner.—Arch. f. exp. Path u. Pharm, B. 68, S. 110, 1912.

8 Cloetta and Fischer.—Arch. f. exp. Path. u. Pharm., B. 54, S. 294, 1906.

9 Albanese.—Centralbl. f. physiol., B. 23, S. 241, 1909.

10 Fickewirth and Heffter.—Biochem. Zeitschrift, B. 40, S. 36, 1912.

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STUDY OF EXPERIMENTAL PURPURA.

BY

E. MARSHALL COWELL, M.D., F.R.C.S.

Although it is now easier to interpret the phenomena of purpura on theoretical grounds, yet it is still difficult to explain many cases clinically, and more difficult to produce the condition experimentally. The experiments generally quoted in the large text-books are those of Grenet, carried out in 1904. This author claimed to have produced purpura in a rabbit by introducing three pathological factors:—(1) Hepatic derangement; (2) nervous disturbance; (3) presence of toxin. These are old observations, but, as far as I know, have neither been confirmed nor disproved. My first experiments were therefore repetitions of Grenet's, using, as far as possible, the same technique.

Grenet took rabbits, ligatured the "hepatic pedicle" for twenty minutes, and then in five to seven days injected an alcoholic extract of serum from a hæmophilic patient into the spinal canal. Paraplegia of varying degrees followed, but within a week ecchymoses on the hind legs invariably appeared. Control injections, without preliminary ligature of the pedicle, and also of simple fluids with ligature, always gave negative results.

In the four rabbits in which these proceedings were repeated, apart from some paresis of the hind limbs, no change was produced. No sign of purpura ever appeared. In six other rabbits intraspinal injections were made to investigate the curious tetaniform convulsions which occur when the cord is irritated. Using any fluid, it was found that 1 c.c., the amount employed by Grenet, caused death in a few minutes; with 0·5 c.c. recovery with paresis followed.

Sections of the liver, cut at different dates after ligature of the pedicle, showed no histological changes.

Charrin in 1892 described the easy production of purpura by injectiug either organisms and their toxins into animals. *B. pyocaneus* was said to give purpura readily. I found, however, that repeated doses of sterilised twenty-four hours' growth of this organism produced negative results. Animals dying with a *pyocaneus* septicæmia even developed no purpura. Other bacterial toxins, as diphtheria and coli, also injected into guinea-pigs, failed to give a positive result. The mere presence of potent bacterial toxins is not sufficient to produce purpura; some other factor must be present before lysis of the capillary endothelium can occur.

The number of blood platelets is said to be greatly reduced in the hæmorrhagic conditions. Duke has studied the subject, and finds the platelet count is 30,000-80,000 instead of the normal 500,000-600,000 per c.mm. Selling, by injecting benzol subcutaneously into dogs, claims to have been able to reduce the number of platelets. It seemed, therefore, that by diminishing the platelets and then giving the animal toxins, that purpura should follow. Unfortunately I have not been able to confirm Selling's work, using rabbits and guinea-pigs. In counting the platelets Pratt's method was used. Careful microscopic examination of the liver and spleen of these animals failed to show free iron. It is difficult to imagine a drug affecting platelets without at the same time destroying the red corpuscles.

Appended are the tables of blood counts, showing the effect of benzol:—

Date.	Hb%	Reds	Whites	Platelets	Inj. Benzol.
Jan. 22	85	5500000	14000	690000	nil.
,, 23	90	6000000	15000	600000	·5cc.
,, 24	—	—	—	—	1cc.
,, 25	85	5160000	11900	480000	1·5cc.
Feb. 1	60	4200000	11250	450000	1cc.
,, 6	90	5060000	15000	500000	1·5cc.
,, 9	80	5000000	12000	550000	1·5cc.
,, 20	90	6500000	12500	480000	1·5cc.
,, 21	95	6800000	19000	540000	nil.
,, Animal killed.					
Jan. 23	99	6500000	12500	460000	·3cc.
24	—	—	—	—	·2cc.
Feb. 1	95	6000000	13000	600000	·2cc.
6	95	6500000	14000	620000	·5cc.
9	98	6000000	15000	580000	·5cc.
10	animal killed.				

CONCLUSIONS.

Unfortunately, as regards the production of experimental purpura, my results have been negative. It appears that the oft-quoted positive experiments must, at any rate, not be accepted at present.

It is doubtful whether benzol has any effect on the blood platelets.

Although the lytic body at work in purpura is undoubtedly allied to a hæmolysin, yet its action seems to be different, since in two fatal cases of purpura neonatorum I have been able to microscope the organs without finding traces of free iron.

BIBLIOGRAPHY.

Grenet.—Archiv. Gen. de Med., Paris, 1904, Vol. I., p. 129.

Charrin.—Compt. Rend. de la Soc. de Biol., Paris, 1902, p. 427.

Duke.—Journ. Amer. Med. Assocn., 1910, Vol. LV., p. 1,185.

Selling.—Ibid., quoted by Duke.

Pratt.—Ibid., 1905, Vol. XLV., p. 1,999.

SOME CLINICAL AND EXPERIMENTAL OBSERVATIONS ON THE OPERATIVE TREATMENT OF FRACTURES,

WITH ESPECIAL REFERENCE TO THE USE OF INTRA-MEDULLARY PEGS.

BY

ERNEST W. HEY GROVES, M.S., F.R.C.S.

The use of intra-medullary pegs for the fractures of the long bones is, of course, no new invention. Various modifications of these, in shape and material, have often been suggested. But for two chief reasons they have not been used very widely, first because of the fear of sepsis being introduced into the marrow cavity, and secondly because of the mechanical difficulty of introducing the peg into the interior of the closely-apposed fractured ends.

Nevertheless I feel quite sure that in certain cases the method of using an intra-medullary peg has great advantages over all others, and I have endeavoured to ascertain, by clinical and experimental observation, the comparative merits of this and other methods of the operative treatment of fractures.

There are two different methods by which the pegs may be introduced into the bone.

1. *Pegs with Fixed Crosspiece.*

Fig. 1 illustrates the first kind of peg. It is comparatively short—*i. e.*, about 2-4 c.m. in length. It has a fixed crosspiece, transfixing its centre, which keeps the peg from slipping too far into one end or other of the hollow bone. One end of the peg is fitted into one fragment of the broken bone. Then the

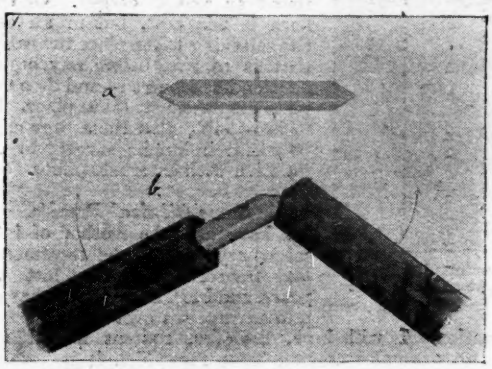

Fig 1.—Short peg with fixed crosspiece. *a*, The peg. *b*, Insertion of the peg. One end has been placed and the other bony fragment is about to be placed in apposition.

other fragment is made to engage the projecting end of the peg, and by bringing the bone fragments into line with one another the muscular tension pulls the ends of the bone together over the enclosed peg, which serves thus to keep the fragments in exact apposition. This type of peg can only be used in those cases where the broken bone fragments are easily separated from one another. When it is in position, owing to its shortness, it does not exercise much influence in maintaining the straight axis of the bone.

2. *Pegs introduced by Wire Traction.*

But in the important class of fractures in which operation is demanded most imperatively; a peg like the one described cannot be employed, simply because in these neglected or malunited cases it is impossible to separate the broken ends of the bone sufficiently to insert the peg. For these cases I have used pegs (Fig. 2) which are drawn into their place by means of wire traction. Each of these pegs is about 4-7 c.m. long, and is perforated in its centre by a hole transverse to its long axis. Along opposite sides of the peg run grooves in its whole length, and into these grooves the ends of the transverse hole open.

The size of the hole and groove is one which easily takes a fine flexible steel wire, which, when threaded through the transverse hole, lies snugly in the grooves, so that the whole peg can be slipped into one fragment of the bone, the other fragment brought into apposition, and then, by traction on the wire, the peg is pulled up into position, so that one half lies in one fragment and the other in the other.

As regards the thickness of these pegs, I have a set of thirteen sizes, varying from one-eighth to one-half inch, each size differing from the other by one-thirty-second of an inch, and also a set of drills for preparing the bone ends. Each drill makes a hole, into which the corresponding peg will slip easily. The object of drilling the bone should simply be to clear away any rough, outstanding spicules of bone, and not to actually enlarge the natural marrow cavity. It is unnecessary to make the pegs fit very tightly, and the less drilling that is done the better. As regards the method of traction on the wire, in most cases it is quite sufficient to pull on the wire ends, which are rolled round pairs of forceps. But if one has to work at the bottom of a deep wound, which makes it difficult to carry this out, the traction may be made by an instrument (Fig. 3), which winds the wire ends on to two revolving shafts, at considerable mechanical advantage

Experimental Results.

I have carried out a series of experiments on cats and rabbits in order to test the rapidity and efficiency of organic union of the bones when fixed after fracture by various

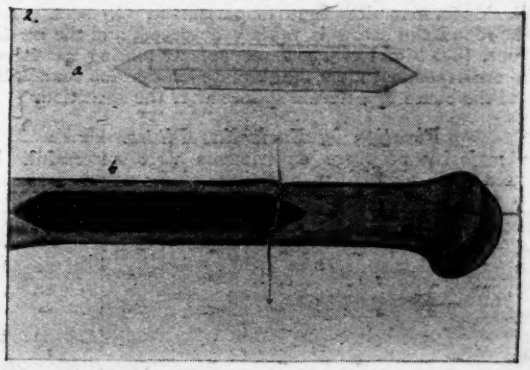

Fig. 2.—Long peg, introduced by wire traction. *a* is the peg showing the groove and perforation for the wire. *b*, Bone, with fragments in line; the peg is wholly in the left-hand fragment, and the wire, the ends of which are indicated by arrows, when pulled on will draw the peg up into the other fragment.

appliances. Up to the present these have been 50 in number, and although my full inquiry is not yet complete, I have attained certain results which are of interest. I will merely give a few examples and illustrations of this part of my work.

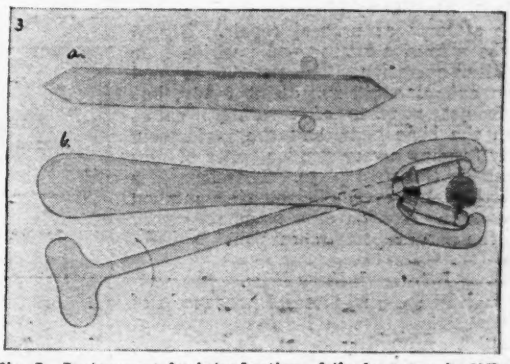

Fig. 3.—Instrument for introduction of the long pegs in difficult cases. *a*, Shows the peg from the side being pulled up by the turning of the two rollers which pull on the wire. *b*, The instrument, which consists essentially in two steel rollers which roll the wire ends, and so pull upon the peg which lies between them.

Plates and Screws.

I endeavoured to carry out the method chiefly advocated by Arbuthnot Lane. That is to say, a small steel plate was used, which was screwed into the dense bone on either side of the fracture without perforating the entire thickness of the bone. But owing to the small size of the screws used, and the fact that it is practically impossible to immobilise the limbs of animals by external splints in the way that it is done in human surgery, none of these experiments gave good results. Invariably within one week

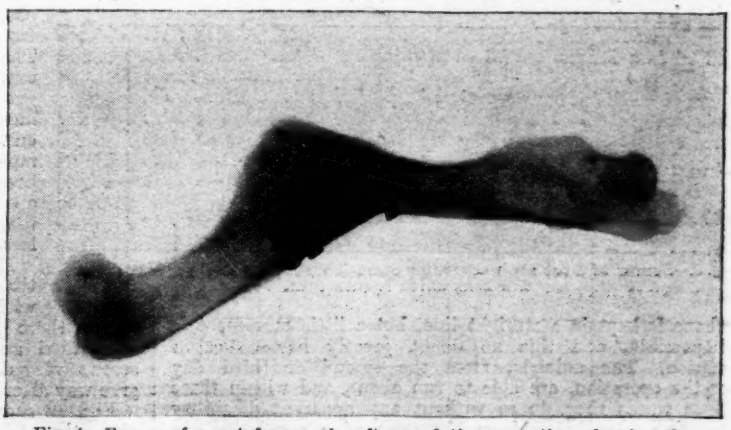

Fig. 4.—Femur of a cat four weeks after a plating operation, showing the way in which the screws from one end have been drawn completely out of place.

of the operation the screws from one or both ends had been pulled out and complete disunion of the fracture occurred. (Fig. 4.) I think that it is fully proved that no screw which merely penetrates the outer shell can hold a bone in position for long if there still exists a constant considerable force tending to its displacement. In such cases the bone round the screw is rapidly absorbed, and the screw comes out. (See Fig. 10.) The success of any plating operation is therefore dependent upon the absolute fixation and immobilisation of the bones by external splints after the operation.

Fixation by Encircling Plates.

The majority of these experiments were successful. I have used steel, aluminium, and silver plates wrapped round the seat of the fracture like a collar and fixed by wire. But in none of these, at the periods during which the animals have been kept alive (four to five weeks), was there any efficient attempt at natural repair. Fig. 5 shows the tibia of a cat in good position four weeks after operation. The position is good and the animal could run about freely, but there is not a trace of reparative callus between the bone ends, and the periosteum is slowly forming a very slender collar of new bone outside the steel plate. Therefore, it is evident that, however efficient that method may be in the fixation of a bone, it greatly retards the natural process of repair.

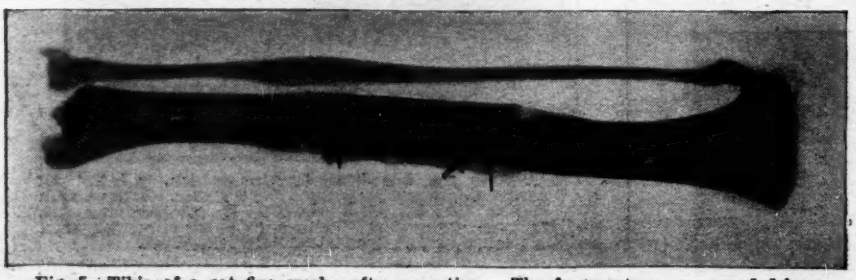

Fig. 5.—Tibia of a cat five weeks after operation. The fragments are surrounded by an aluminium plate which is almost transparent to the *x* rays. With the exception of a little periosteal callus at each end of the plate, there is no trace of repair.

Intramedullary Pegs.

Although I have used internal pegs made of various materials, I will here only speak of those which are unabsorbable. In comparing the merits of various materials—*e.g.*, bone, steel, and aluminium—the metal pegs are much the best for animal experiments, because those made of bone or ivory are very brittle, and are liable to be broken. It is an interesting fact, as illustrating the very powerful force which tends to displace a fractured bone, that in every case (about ten in number) in which I used a bone or ivory peg for the tibia or femur of an animal, this was snapped in two, and the fragments thus became displaced. Steel is preferable to aluminium, because it throws such a good shadow by the X-Rays. A steel peg is very easy to insert by the method I have described. When it is inserted the bone is kept in a straight line, but a little movement is still possible, and this no doubt greatly hastens callus formation. The animals, from the second or third day after the operation, are able to run about, and within three or four weeks they do so without any apparent disability or limping. Fig. 6 is from the tibia of a cat killed six weeks after the operation. The experiments demonstrate that the method is easy, it secures good position, firm and speedy union, and perfect functional result.

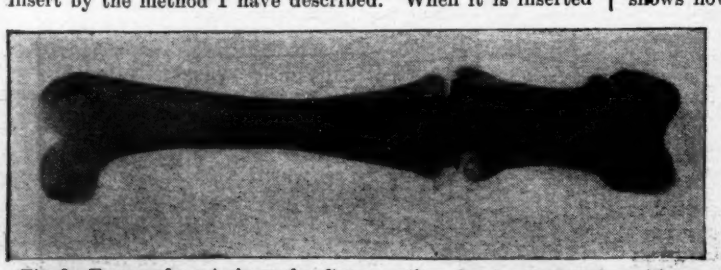

Fig. 6.—Femur of a cat six weeks after operation, showing steel peg in position The bony union is quite firm.

Magnesium has a very definite and distinct characteristic when used for the pegging of bones. It is absorbed within a few weeks, and in the process of its absorption it greatly stimulates the callus production and the ossification of the same. Fig. 7 shows the tibia of a cat eight weeks after the operation, and it will be seen that there is a well-marked spindle-shaped thickening of the bone at the seat of the fracture.

Although this property of magnesium renders it unsuitable for use in ordinary cases, it may be of great value in treatment of old cases of ununited fracture, in which a powerful stimulus to callus formation is the object most desired.

Of the other forms of absorbable bone pegs I have not yet found any which give good results with animal experiments. I have used decalcified bone, horn, catgut, gelatine hardened with formol, and fibrine round an axis of catgut. Of these horn is the best, but in all cases the softening takes place too early—that is to say, before any organic fixation has occurred—and then complete displacement takes place. But it is possible that these pegs might be useful in human surgery in conjunction with external splints.

Some Clinical Results.

I have used the method of intramedullary pegs for the treatment of bad fractures for the last two years, and I feel sure that it has certain advantages over other methods of operation. I will here, however, content myself with giving two examples of conditions for which it is specially suitable. Fig. 8 shows the femur of a boy of 13, broken in its upper third, with a displacement which neither weight extension nor a double-inclined plane could reduce. Fig. 9 shows how perfect the apposition was after the introduction of an intra-medullary peg. This is the kind of fracture for which the method is especially suitable. The displacement is very difficult to correct by external means; it is still more so to maintain in good position. The bones lie so deeply bedded in muscles that any kind of screwing operation is difficult to carry out efficiently, and, moreover, the muscular tension is so great that a single plate becomes displaced within a short time after the operation. Fig. 10 is from a case of this kind, and it shows the plate and screws, which have become loose within two weeks of the operation.

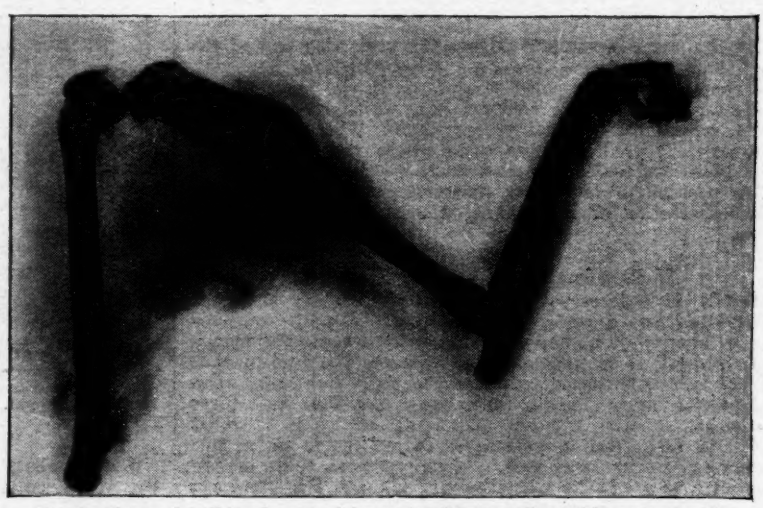

Fig. 7.—Shows the tibia of a cat, eight weeks after operation, with a peg of solid magnesium. There is marked over-production of callus.

Fig. 11 illustrates a malunited fracture of the tibia and fibula 18 months after the accident, with much overlapping of the fragments. In such a case there is very great muscular tension when the bone is divided and replaced. This would make fixation by screws very difficult, and such fixation would be liable to give way after the operation.

Fig. 12 shows the same case four weeks after the operation, the ends of the tibia being replaced over an intramedullary peg.

Conclusions.

The advantages of the intra-medullary peg method, in operative treatment of fractures may be summarised as

follows:—(1) It can be easily and quickly performed, provided the details I have suggested are observed. (2) It requires a very much smaller incision and less exposure of the soft parts. (3) It involves a minimum of injury to the periosteum. (4) It adjusts the broken bone in the correct position. (5) It allows slight movements between the fragments, which is beneficial for rapid union. (6) It makes the use of external splints unnecessary after the operation, and massage and movements are thus rendered possible as soon as the wound has healed.

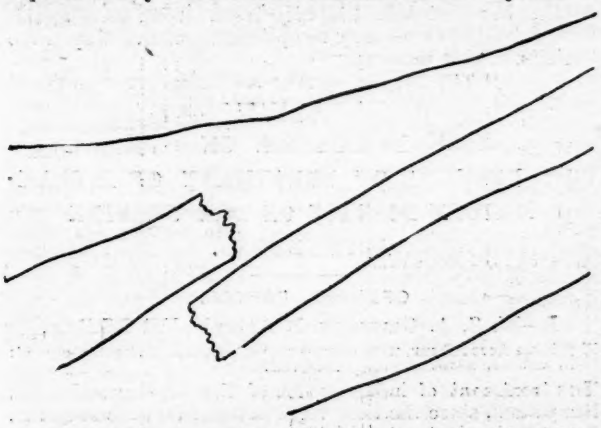

Fig. 8.—Fractured femur of a boy of 13, broken in its upper third, with displacement of its upper fragment in front of the lower.

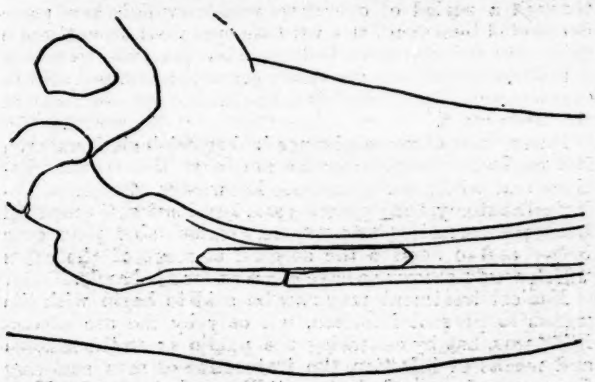

Fig. 9.—The same case shown in Fig. 8, a fortnight after the introduction of a bone peg.

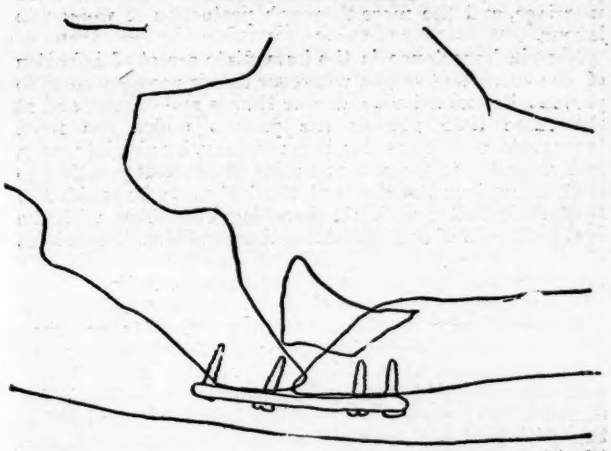

Fig. 10.—Femur of a man, broken in its upper third, showing how a plate and screws failed to maintain the correct position of the bones.

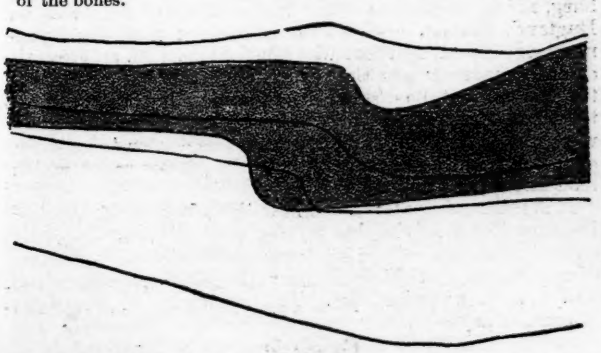

Fig. 11.—Malunited fracture of the leg bones eighteen months after the accident. The tibia is shaded and the fibula only shown in outline.

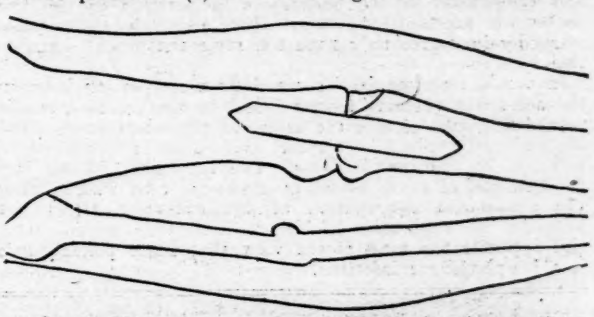

Fig. 12.—The same case as shown in Fig. 11, after the operation, showing the peg in position. A piece of the wire used to draw the peg into position has been left to prevent the peg slipping up or down.

[Figures 8, 9, 10, 11 and 12 are accurate outline drawings from the actual skiagrams, reproduced in this form for the sake of clearness.—ED.]

THE ACUTE AND SUBACUTE LESIONS INDUCED IN THE SPINAL CORD BY INFECTION OF ITS LYMPH SYSTEM.

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The research carried out during the past year has been a continuation of the investigations on the inflammatory phenomena induced in the central nervous system by infection of its lymphatic system. In our last paper we gave in detail the histological evidence which demonstrated that the path of the toxic lymph could be traced, by the presence of inflammatory reaction, along the infected sciatic nerve as far as the posterior root ganglion, and for a considerable distance along the spinal roots. The nerve had been infected by placing a celloidin capsule containing a broth culture of micro-organisms in contact with it.

In our most recent experiments the spinal cord was infected in two ways—either by removing the laminæ of the vertebræ and suturing the capsule against the dura mater at the level of the lumbar enlargement, or by separating the muscles from the vertebræ in the dorsal region and placing a capsule against an intervertebral foramen.

All the experiments yielded positive results, but there were marked differences in the degree of the reaction, because in some cases the capsule had burst and a growth of organisms had occurred in the tissues and spread along the dura mater.

In the experiments in which the capsule had burst the changes in the spinal cord were of an acute nature. When there was no growth of organisms outside the capsule the reaction was sub-acute.

SYNOPSIS OF THE PHENOMENA IN THE ACUTE SERIES.

1. In the epidural tissue there were micro-organisms, polymorphonuclear leucocytes, and large lymphocytes.
2. Inflammatory reaction of the cells of the dura mater and pia mater, with the formation of polyblasts.
3. Within the spinal cord were found:—Proliferation of the adventitial and endothelial cells, with the formation of polyblasts, and here and there of compound granular corpuscles; dilatation of the blood vessels, and the formation of thrombi within them; neuroglia proliferation; nerve cell degeneration and neuronophagy.

SYNOPSIS OF THE PHENOMENA IN THE SUB-ACUTE SERIES.

1. In the epidural tissue there were polymorphonuclear leucocytes, plasma cells, and polyblasts.
2. Plasma cells were present also in the dura mater and in the capsule of the posterior root ganglia; in the substance of the posterior root ganglia, and collected into a prominent group at the proximal pole; in the perineurium of the spinal roots and in the adventitial sheath of the vessels in it; in the

pia mater and in the adventitial sheath of the vessels passing into the spinal cord.

3. Proliferation of the neuroglial cells.

A comparison of the phenomena of reaction in the two series of experiments shows that although they differ markedly in degree they have the same anatomical distribution.

In our last communication we drew a comparison between the sub-acute vascular lesions found in the infected sciatic nerves and those seen in the brains of general paralytics, and we assigned to both a similar mechanism of production—infection of the lymph paths. The induction, in our last experiments, of acute lesions in the spinal cord which follow the anatomical distribution of the sub-acute series has widened the application of the research very considerably, and supports the most recent view that acute poliomyelitis is a lymphogenous infection.

EIGHTIETH ANNUAL MEETING

OF THE

British Medical Association.

Held in Liverpool on July 19th, 20th, 22nd, 23rd, 24th, 25th, and 26th.

PROCEEDINGS OF SECTIONS.

SECTION OF GYNAECOLOGY AND OBSTETRICS.

Professor Henry Briggs, M.B., F.R.C.S., President.

PRESIDENT'S INTRODUCTORY REMARKS.

Professor Briggs, after extending a cordial welcome to members and visitors, continued: The prospect of three days' full employment on subjects of immense scientific and practical importance is consistent with the traditions of these annual gatherings of the Section and with the steady progress shown throughout the history of obstetrics and gynaecology. Discouragements from any source or of any kind looming, or threatening to loom, in obstetrics must disappear under the combined influences of healthy mothers and children and an efficient obstetrical service. The latter requires the extension in populous centres of adequately equipped training schools in obstetrics for the medical student, the midwife, and the monthly nurse. A short statement of our local obstetrical history—in 1796 a ladies' charity, in 1841 a lying-in hospital, in 1869 an amalgamation of the two, at a date soon after the discoveries of anaesthetics and antiseptics—reveals an ancient institution which has outgrown its present, too small and always overcrowded, accommodation for 24 in-patients amidst an average of 2,000 out-patients yearly attended at their homes. On a central site, already acquired by the committee, a modern Maternity Hospital of 85 beds, by the great generosity of Sir William Hartley, is, at an early date, to be built. The endowment fund provided by other friends of the hospital has now reached the sum of £15,000; another £5,000 at least is desired. The gratifying assurance of this vigorous accession to the growth of our own local obstetrical training school will encourage the ever-increasing efforts of the hundreds of medical men and midwives in the district of Liverpool for the advancement of this branch of practice.

In addition to our membership of the British Medical Association and of our own purely local medical societies, we in this district, as obstetricians, are all members of the North of England Obstetrical and Gynaecological Society; its monthly meetings in one or other of the four university centres of Manchester, Liverpool, Leeds, and Sheffield, are well attended. To recall the society's origin in 1889 is to recite one of a long roll of varied and valuable public services, capably and courageously rendered by our neighbour, Sir William Sinclair, of Manchester, who has this year resigned his seat on the Central Midwives Board after ten years' faithful service. We know his enormous energy; we recognize the gigantic debt obstetrics now owes to him.

The activity and the ability of the Royal Society of Medicine have amply compensated for the fusion of the London societies; the Obstetrical and Gynaecological Section has maintained its high level. With such organizations behind us we may be optimistic, and thus open the business of this meeting.

DISCUSSION ON THE RESULTS OF TREATMENT OF INFLAMMATORY DISEASE OF THE UTERINE APPENDAGES.

OPENING PAPERS.

I.—W. S. A. Griffith, M.D.Camb., F.R.C.S.Eng.,
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The treatment of inflammation of the ovaries and tubes has passed, since the time when many of us were students, from a simple treatment of symptoms, based on a very imperfect knowledge of the pathology of the disease, through a period of operative surgery which has since dominated treatment, and with the progress of antiseptic principles and operation technique has gradually achieved a high degree of success, to the great benefit and relief of many chronic and formerly incurable, as well as many of the acute cases.

During recent years advance in bacteriological research has enabled us to discover the causes of these inflammations, and with a more accurate knowledge of their pathological anatomy, their progress and termination, the medical treatment promises to prove of greater value than ever before, and to deprive the surgeon of some of the cases which would otherwise have come under his hands.

Medical treatment may now be said to begin with the means to prevent infection, not only by the use of disinfectants, but by educating the public as to the sources and means of infection, the instruction of men suffering from gonorrhoea and gleet as to the suffering and misery produced by the communication of the infection after marriage, and the more thorough protection of women in labour.

Next in importance is the immediate arrest of infection of the vulva and vagina wherever this is possible, so as to prevent its extension to deeper tissues and organs, and at the same time placing the patient under the most favourable conditions for her recovery, by absolute rest, and attention to the common rules of medical practice, so that the various structures of the body may be enabled to fight their battle with the invading organisms with the best prospects of success, aiding them with such agents as serums and vaccines and such other methods as radiology and pharmacology provide for us from time to time.

It is of great interest to look back at the writings of those who were leaders in gynaecology in our student time, when the first investigations into the use of antiseptics were being made, and when the fear of opening the abdomen was only equalled by our ignorance of the condition inside it, and of the proper methods of treatment. If we look at the writings of such men as Matthews Duncan and Charles West in this country; Courty, Professor of Clinical Surgery at Montpellier; Scanzoni, Professor of Medicine at Würzburg, as representative of the best practice of their time in England, France, and Germany, we see that the treatment advocated by them was almost identical, and almost entirely medical; and that even in cases of pelvic abscess there was great hesitation in recommending any surgical treatment, with the result that large numbers of women were unrelieved and wandered from one doctor to another, or from one cure to another, unable to get the relief which modern surgery now affords.

It is interesting also to see how the advance of surgical treatment of the living led to, and was followed by a more exact knowledge of the anatomy and pathology of these diseases than was obtained from clinical observations and *post-mortem* examinations. The principles of treatment advocated at that time were common to all countries, and it may be of interest to see how far they are maintained at the present time. First in importance, then as now, were the common-sense measures—lying in bed, attention to the

bowels, and properly regulated diet. Without these any other treatment can have a poor chance. Next in importance came "local depletion" advocated by all the older writers, and now almost, if not entirely, given up. This was carried out by the application of leeches, a dozen to that part of the abdomen where pain was most acute. A better and more effectual method was the application of a smaller number, from four to six, to the cervix by means of the speculum, the external os being closed by a small plug of cotton-wool to prevent them entering the uterine cavity. The objects of the depletion were the relief of pain and the diminution of the congestion of the part affected. They undoubtedly gave relief, and when necessary the application of leeches was repeated in smaller numbers. These were followed by poultices, hot compresses, with opiates for the relief of pain. Aperients and mild febrifuges were also given. Later, especially in more chronic cases, "resolvents" such as mercurial and belladonna ointments, and blisters were applied, and a more stimulating diet was given with wine and tonics, of which liquor cinchonae was a favourite with many.

These general lines of treatment, which varied in detail in the hands of different physicians, indicate the practice of medicine at that time. They were good common-sense principles, handed down from our forefathers, untainted by a knowledge of microbes, serums, and vaccines, or of the risk of septic instruments, fingers, and clothes. They are in the main the principles of treatment which are carried out by the great body of practitioners at the present time.

I have details of 48 cases in the gynaecological wards of St. Bartholomew's Hospital, treated in the four years 1907–1910 without operation, and without serum or vaccine treatment. They show the usual results of the ordinary routine treatment—no deaths, and the local condition much improved.

Thermal Treatment of Pelvic Inflammations.

The beneficial results of hot poultices and fomentations to the abdomen in pelvic inflammation has led to attempts to apply heat more directly through the vagina. Daily douches at a temperature rising from 115° F. to 122° F., and continued for from ten to fifteen minutes, has been strongly recommended by Richelot.[1]

S. Flatou, of Nuremberg,[2] describes a vaginal heating instrument (but gives no detail of cases) by which he says he can raise the temperature of the pelvis up to 40° C.

Clavigo[3] reports cures of cases of various pelvic inflammations.

The continuous hot douche is the only form of which I have had experience, but I am unable to say how much benefit is obtained from it. If the vulva is well protected, it certainly gives more relief than the ordinary douche which is so commonly ordered by medical men for every form of gynaecological ailment. The value of the ordinary douche is extremely limited, and the benefit obtained from it small in proportion to the time and trouble which its frequent use entails.

Radium Treatment.

Treatment of pelvic inflammations by radium is still in a stage of trial, and we are indebted chiefly to France for such information as we have. My personal experience is limited to one case treated at the Radium Institute in June of this year, and it is too early to form any opinion as to the result.

Cheron[4] records 26 cases, described as chronic, with grave lesions, which did not yield to former treatment. Two of these, however, were cases of so-called ovarian pain, without evidence of inflammation, and as might be expected were not benefited by the treatment. The remainder, he says, recovered, and remained well during many months. Seventeen of these were bilateral, of which eight were severe on one side and slight on the other. These, he says, are perfectly well and menstruation remains normal. There were nine cases in which the bilateral lesions were well marked which also did well, some with a diminution of menstrual flow, others with entire cessation for two or three months, but menstruation then returned normally; and in such cases he remarks that we must not too hurriedly conclude that menstruation has ceased permanently. Seven cases were unilateral; two of these were cured and preserved their ovarian functions. He draws the conclusion that we should always try the effects of radium treatment before having recourse to the removal of the uterus or the appendages.

Madame Fabre, M.D.,[5] says of this treatment:

> In certain cases of chronic inflammation of the uterine annexes, with great infiltration and perimetritis, we notice a softening of the adhesions and a diminution of the mass of the annexial tumours. The most noticeable phenomenon, however, is always the diminution and subsequent complete suppression of pain. The complete cure is slow. We may affirm that the ultra-penetrating radiations of radium have a curative action in all inflammatory diseases of the pelvis with the exception of genital tuberculosis, of which there is not yet sufficient evidence.

Jacobs, of Brussels,[6] relates two cases of chronic salpingitis, in which the application of radium appeared to have some beneficial effect. The disease was left-sided in both, and he used 3 cg. of radium applied through the left fornix of the vagina—in one, for six hours on alternate days for a period of two months, in the other, every third day for four weeks. At the end of the treatment the tubes were no longer tender, and in one case the inflamed tube was diminished in volume. He does not speak highly of the results.

The latest contribution is by Barcat, Assistant at the St. Louis Hospital, Paris,[7] in which he gives a short summary of recent work on the use of radium, and in the case of salpingo-oöphoritis speaks highly of the results. There are no details, however.

There appears to be no evidence that radium is of value in acute cases, nor in those in which there is any considerable formation of pus. The application of radium should be made, as far as possible, in the vagina, in some cases in the cervix, and Cheron especially points out the uselessness, and even the danger, of introducing radium into the body of the uterus in cases of inflammation.

In a recent paper by Professor Bordier of Lyons, at the Royal Society of Medicine,[8] "On the Treatment of Uterine Fibroids by the *X* Rays," he laid stress on his successful results being due to the direct effect of the rays destroying the follicles and the ova.

On these grounds I felt bound to criticize the treatment of fibroids by radiology as compared with the removal by operation, in which the ovaries, if healthy, are left untouched and the risk of a sudden climacteric disturbance is avoided. One of the points, therefore, in which radiologists will have to exercise caution, especially in the minor inflammatory affections, is the risk of sterilizing the patient as a result of this treatment. Owing to the great expense and the difficulty of obtaining radium, it is improbable that it will be frequently used for this class of cases.

Serums and Vaccines in Pelvic Inflammations.

One of the great advances in the treatment of all inflammations has followed the systematic bacteriological examination of the infection. This is a comparatively simple matter where the affected part is external or communicates directly and closely with the exterior. Both the Fallopian tubes and ovaries are unfortunately too far away, and often it is impossible, until the organ is exposed by operation, to ascertain with certainty the nature of the infecting agent. In cases of inflammation of these organs, examination of the bacteria of the vulva would give no reliable results. Those of the vagina and cervix, especially if gonococci or streptococci are present, would be of more value, but their presence is not conclusive evidence as to the nature of the infecting agent in the more distant parts.

It is held by some authorities that an acute vaginitis is necessarily the result of gonococcus infection, and until the systematic bacteriological examination of these cases was undertaken, most of us, I suppose, held the same opinion. This, however, is clearly not true, and, indeed, some of the most chronic and troublesome cases, apparently not amenable to any chemical antiseptic, have been those in which the pus contained no definite pathogenic organisms—only a variety of forms from which no useful vaccine could be obtained.

One can expect a practical clinical cure of gonorrhoeal vaginitis in from one to two weeks by chemical disinfection

done. I would not undertake to cure the other cases in three months. Now these latter cases fortunately appear to have no tendency to spread beyond the cervix, whereas the gonococcal and streptococcal infections are those which most certainly are the cause of the great majority of inflammations by extension from the uterus, and this extension is most prone to take place in connexion with menstruation, abortion, or labour, and it is imperative if either of these is impending that the vagina be adequately disinfected. The general recognition by the profession of the importance and simplicity of vaginal disinfection in these cases, and of the necessity of warning young men who have suffered from gonorrhoea of the risk of marriage and of infecting their wives (not necessarily by an acute infection readily ascertained, but by a slow gradual process, the result of an uncured gleet, which rarely produces any obvious symptoms until it has reached the tubes, ovaries, and peritoneum)—the recognition, I say, of these two great factors would do a very great deal to diminish the number of such cases and prevent much ill health and sterility.

When the uterus itself is infected, or, what is more common, when its mucous membrane is in a state of chronic inflammation as a result of infection, it can be disinfected by the careful use of the flushing curette and the swabbing afterwards by an efficient antiseptic, and in cases where the Fallopian tubes are also the seat of chronic inflammation, this treatment of the endometrium appears to act most beneficially on the mucous membrane of the tubes, in a way similar to that obtained by the removal of tonsils and adenoids on the Eustachian tubes and the ear.

Treatment by serums and vaccines is being carried out by numerous investigators, but much information is still needed before definite rules can be formulated. It is obviously essential that in the cases of pelvic inflammation a competent gynaecologist be associated with a competent bacteriologist in order that reliable information may be obtained. A long list of investigators might be quoted, but it seems in the short time available better to point out certain matters which are peculiar to pelvic inflammations and add to the difficulty of the investigation and to the uncertainty of the results.

It is in the first place impracticable to entirely separate inflammation of the tubes and ovaries from inflammation of the pelvic peritoneum and cellular tissue; they must be considered together, their treatment is identical.

With the exception of the tuberculous group, most pelvic inflammations arise from infection which extends through the vagina and uterus to the tubes and peritoneum or to the cellular tissue, lymph and blood vessels of the pelvic diaphragm, or in both of these ways.

Mr. Girling Ball, in his Hunterian lecture,[9] pointed out that in all streptococcic infections it is probable that some bacteria invade the blood stream, but in many cases they are destroyed by the serum and the leucocytes, so that they are not found in the circulating blood, natural immunity being sufficient to prevent general infection.

We may regard infection as the entry of bacteria to a part, and, unless they are destroyed there and then, they multiply with enormous rapidity and either remain localized or they spread along the surface of the part infected, or through a wound they enter lymphatic or blood channels, spreading in all directions, unless arrested by natural immunity, which varies in every individual, and in the same individual it varies in its power of resistance to different organisms.

The use of serum is to provide an immediate antidote to the bacterial toxins, and unless used early it appears to be of little use. Vaccines we may regard not as direct antidotes to the poison, but as stimulants to the tissues to provide an extra quantity of their natural antidote, and unless these tissues are already overcome by the virulence of the bacterial invasion, and have exhausted their power of production of antibodies, the injection of a vaccine prepared from the invading organisms appears to be able, under favourable circumstances, to turn the scale and arrest the further bacterial growth and the production of toxins (this is termed "active immunity").

If this correctly represents the present views of this difficult and very complex subject, it appears probable that the use of vaccines in gynaecology will prove to be of most value as a prophylactic against extension of infection, and in a manner somewhat similar to our usual methods of chemical disinfection. Chemical disinfection is of the greatest value when carried out thoroughly, and applied, as it easily can be, to the infected vagina, and less easily, but with fair efficiency, to the infected uterus.

The value of thorough disinfection of the vulva and vagina, which are usually the first seats of infection, cannot be overestimated, and many a woman might be saved from extension of inflammation to the uterine appendages if this were more generally recognized and carried out without delay.

It is as futile to attempt to disinfect the vagina by the use of the ordinary douche as it would be to prepare one's hands for an operation by squirting an antiseptic on them.

In some forms of inflammation in little children and girls in whom the infection has extended beyond the hymen, and in whom efficient chemical disinfection is to be avoided if possible, even under an anaesthetic, a vaccine appears to be capable of arresting the disease.

The recent paper by Chapple[10] on two cases of pneumococcal vulvo-vaginitis in young girls is important evidence of this.

Butler and Long[11] record several cases of gonorrhoeal vulvo-vaginitis in children, treated by vaccine with good results.

We contribute from St. Bartholomew's Hospital 3 cases of gonorrhoeal vaginitis, 1 in a child of 5, 2 in adults, in which gonococci were identified by film and culture. The two adults were treated by chemical disinfection under anaesthesia, the child was merely irrigated with Condy's fluid. All three received an autogenous vaccine in two doses of 10 million at intervals of three to four days. In each case, after fourteen to eighteen days, no gonococci could be found.

A pregnant woman (nine months) with acute streptococcus vaginitis was disinfected with 2 per cent. silver nitrate, and received two doses of 10 million autogenous vaccine. She was confined twenty days later. The puerperium was normal, and the infant's eyes were not affected.

There are 13 cases of chronic pelvic inflammation at present under treatment with autogenous vaccine, prepared from organisms obtained from the uterine cavity:

Streptococcus pyogenes	10 cases
Gonococcus	1 case
Diphtheroid bacillus	2 cases

on which we hope to report later.

Records of tuberculin treatment in cases of pelvic inflammations are very scanty. A case was recorded by Miss Ivens[12] after the removal of the tubes.

The paper by Heinsius[13] is a contribution to the vaccine treatment of gonorrhoeal inflammation of the uterus and appendages. His 10 cases are interesting, but they are not sufficiently in detail to enable us to accept his conclusions unreservedly. The difficulty of distinguishing *post hoc* from *propter hoc* results is always present, and, as several of his cases show (though they are described as gonorrhoeal), gonococci were not always found or are not reported as found; one of the great difficulties in the investigation and adoption of vaccine treatment is the doubt whether the organism obtained from the vagina and cervix, from which the vaccine is usually prepared, is really the organism of the more distant pelvic inflammation.

Again, in chronic cases much of the induration felt in the neighbourhood of the uterus is scar tissue, and the bulk of the mass is often increased by the loaded condition of the bowels. We must all be aware that it is not rare for the diminution of these masses to be attributed to the cure of inflammation, or to the rapid disappearance of a tumour, rather than to the emptying of the bowel, which is often the true explanation.

We may, however, agree with Heinsius that treatment of gonorrhoeal inflammations of the uterine annexes by vaccine is a therapeutical measure of value, and is worth a more extensive trial.

There is a general consensus of opinion that the use of vaccine must be subservient to the use of the best surgical methods; the septic vagina or uterus must be cleaned

out, and the opening of abscesses and their drainage are just as essential as ever they were.

I have had under my care chronic cases of infective endometritis, with thickened tubes, in which the disinfection of the uterine cavity by the flushing curette, and the free application of iodine has been followed, not only by the cure of the endometritis, but by a recovery of the tubes so perfect that in one case a patient previously sterile gave birth to a child at the end of twelve months after the curetting.

References.

[1] *La gynécologie*, May, 1909, p. 193. [2] *Medical Record*, 1911, vol. lxxx, p. 175. [3] "Action of Hot Air in Gynaecology," British Medical Journal, Epitome, August 19th, 1911, par. 27. [4] Du traitement des salpingo-oöphorites, etc., par le rayonnement ultra-pénétrant du radium, *L'Obstétrique*, 1909, p. 818. [5] *Archives of the Roentgen Rays*, November, 1910, p. 228. [6] *Le Radium en gynécologie*, 1911, p. 54. [7] *Précis de radiumthérapie*, Paris, 1912. [8] *Proc. Roy. Soc. Med.*, vol. v, No. 6, p. 115. [9] *Lancet*, June 8th, 1912, p. 1515 [10] Ibid., June 22nd, 1912, p. 1685. [11] *Journ. Amer. Med. Assoc.*, 1908, vol. i, p. 744. [12] *Journ. of Obstet. and Gyn.*, vol. xvi, p. 424. [13] *Monat. für Geb. u. Gyn.*, 1911, vol. xxxiii, p. 426.

II.—Christopher Martin, M.B., C.M.Edin., F.R.C.S.Eng.,

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I have had the honour of being asked to discuss the results of the surgical treatment of inflammation of the uterine appendages. The subject is a complex one, and in the limited time at my disposal I can only touch on some of the salient points.

At the outset, let me say I am not an advocate of surgical treatment for all or even the majority of cases. The various medical lines of treatment must be given a full and patient trial, and an operation recommended only when they fail, or it is obvious they are futile.

In judging of the value of an operation we must bear in mind:

1. The natural course of the disease untreated.
2. The results of medical treatment.
3. The mortality of the operation.
4. The effect on the local disease.
5. The effect on the general health.

In each case we must count the cost and estimate what the patient stands to gain or lose by the proceeding. Will she be really the better for it? Are the probable benefits worth the risk? If we cannot make her any better, let us take care we do not make her any worse. In past years many needless mutilating operations have been performed, but nowadays saner and more conservative methods prevail.

It will be convenient to consider ovarian and tubal inflammation separately.

A. *Ovarian Inflammation.*

Acute Ovaritis.—The condition of acute ovaritis, except as part of a tubo-ovarian infection, seldom calls for surgical treatment. It rarely goes on to abscess, and I do not consider it justifiable to remove an ovary for acute inflammation unless there be suppuration. It would be just as reasonable to remove the testis for acute orchitis.

Occasionally, however, suppuration does occurs in the substance of the ovary; and then the surgeon has to interfere. If the ovary can be felt in the pouch of Douglas, distended into a bag of matter, it is best opened and drained per vaginam by an incision in the posterior fornix. This allows the pus to be evacuated at the most dependent point, with the least disturbance of the tissues, and the minimum risk to the patient. With the opening of the abscess the febrile symptoms subside, and the patient is able to overcome the general toxaemia. In many cases a permanent cure results. But if it does not, and the ovary continues so painful, swollen, and adherent that its removal is called for, this second operation is done under much more favourable circumstances and with much less risk to the patient.

Chronic Ovaritis.—There are many varieties and degrees of chronic inflammation of the ovary. In some it is small, hard, anaemic, and sclerosed. In some it is enlarged, congested, and tender. In others it becomes cystic—the contents of the cysts being serous, colloid, or altered blood. From time to time the cyst contents leak into the peritoneum and set up an adhesive inflammation, which glues the ovaries to the uterus, the tubes, the bowel, and broad ligament. These patients suffer from recurrent pelvic peritonitis, dysmenorrhoea, and general pelvic distress. It is usually necessary to remove such diseased ovaries. The operation is sometimes a difficult and dangerous one from the density of the adhesions. The more grossly diseased and adherent an ovary is, the greater is the justification for its removal; but the greater also is the risk.

Less severe cases of cystic ovaritis can often be cured by an operation short of removal—namely, separation of adhesions, ignipuncture of the cysts, or resection of the diseased part of the ovary. In the case of young women such conservative operations should be given a fair trial, and both ovaries removed only where the patient's sufferings are severe and milder measures fail. If but one ovary be diseased, this should be removed and the healthy one left. Even if only a small part can be saved, this is enough to produce the internal secretion (so essential to the patient's well-being), menstruation continues, and the possibility of pregnancy remains.

The removal of the ovaries for "cirrhosis" is seldom satisfactory, and often does more harm than good. Cirrhosis is usually the result of premature senile changes in the organ. It may, however, be caused by repeated attacks of inflammation. The ovary becomes hard, nodular, and fissured. The scar tissue may compress the nerve fibres in its substance, and cause severe neuralgic pain. Its removal is then sometimes necessary. But if the other ovary be healthy, it should be preserved, and not sacrificed on the plea that "it may go the same way."

In general, the more severe the local lesion in the ovary, the more satisfactory is the after-result of removal, and vice versa. To remove healthy ovaries for simple dysmenorrhoea, or purely nervous disorders (such as epilepsy, insanity, and nymphomania), is quite unjustifiable.

It is futile to perform double oöphorectomy in the hope of curing the "chronic neurotic." We all know her—the thin, fretful, hysterical creature, who wears out the patience of her doctor and nurses, and makes her husband's life miserable. She goes from consultant to consultant and falls an easy victim to the quack. In turn, she tries all the fashionable remedies. At one time she has a Weir Mitchell "rest cure"; at another a course of "high frequency." One physician washes out her stomach, which he finds dilated; another has her colon irrigated for a supposed colitis. She then tries the surgeons, who perhaps short-circuit her stomach, remove her appendix, or stitch up her kidneys. Sooner or later she falls in the hands of a gynaecologist who treats her with pessaries, curetting and ventrifixation. Finally, he removes her ovaries, and the last stage of the patient is worse than the first.

Remote Results of Removal of both Ovaries.—The remote results of removal of the ovaries vary with the age of the patient. If she be a young woman the changes are more marked than if she be middle-aged. Menstruation (with few exceptions) ceases, and the whole genital canal undergoes atrophy. The uterus may return to the infantile condition. The vagina shrinks and loses its elasticity. There is often a curious tendency to develop urethral caruncle and painful vascular patches around the vaginal orifice. There is, of course, complete sterility. In many cases the sexual instincts dwindle and disappear. Coitus may become distasteful from psychical causes, or distressing from physical changes. She suffers severely from all the sympathetic disturbances of the menopause, such as heats and flushes, palpitation and giddiness. Usually she becomes fat. But I have not seen any tendency to the development of male characteristics, such as hair on the face, deepening of the voice, or mannish habits and modes of thought. These masculine features depend on the presence of the internal secretion of the testes, and not on the absence of that of the ovaries. Now, none of these climacteric changes occur if only one ovary be removed, or even a small fragment—not larger than a cherry stone—be left behind. They can to a certain extent be prevented or diminished by administering to the patient the ovarian substance of the sheep. But this never quite takes the place of the natural secretion. It is probable that the ovarian secretion of the woman differs somewhat both in chemical

composition and physiological effects from that of other animals.

B. *Tubal Inflammation.*

The results of surgical treatment of tubal inflammation depend on the cause of the disease, on the severity of the inflammation, and on the extent of the operation. The three chief causes are gonorrhoea, tubercle, and sepsis. Salpingitis nearly always spreads from the uterus by continuity of mucous membrane; and therefore it indicates a past and usually a present endometritis. Developmentally the uterus and tubes are one; and in many pathological processes they must also be considered as one organ. If the tubes be infected the uterus is also infected.

Salpingitis may be acute or chronic, mild or severe, single or double, and the line of treatment varies accordingly. Mild cases do not call for operation, and can be cured by medical treatment.

Adherent Appendages.—In most cases of salpingitis there is more or less pelvic peritonitis causing adhesions between the tubes, ovaries, uterus, bowel, and omentum. In recent acute cases these adhesions are soft and easily parted. In old cases (for example, in bad chronic pyosalpinx) they may be extremely dense and difficult to separate. In many cases of adherent appendages it will be found possible to remedy matters by a conservative operation. The abdomen is opened, the adhesions are broken down, and the tubes and ovaries freed. If the abdominal end be closed, by gluing together of the fimbriae, it can be opened. If the tube be distended with serous fluid (as in hydrosalpinx), it may be slit up, the contents evacuated, and the ostium reformed. If this proceeding be combined with curetting and ventrifixation, a cure usually results. A patient previously sterile may be made fertile. I know of many cases where pregnancies have followed these conservative operations.

Pelvic Adhesions Secondary to Appendicitis.—It must be noted that in many cases of adherent appendages the pelvic peritonitis is secondary, not to salpingitis or ovaritis, but to a previous appendicitis. The latter sets up a local peritonitis. The inflammatory effusion sinks into the pelvis—the most dependent part—and covers the appendages with plastic lymph, which sets into firm adhesions. In such cases there is no justification for removing the ovaries and tubes. The appendix should be taken away, the appendages freed from their toils, the fimbriated ends reopened, and the uterus stitched forwards. The results are excellent; the pelvic pain and dysmenorrhoea disappear, and patients previously sterile may now conceive. When operating for adherent appendages or adherent retroversion, it should be a rule always to examine the appendix and remove it if there be any signs of old inflammation.

Unilateral Salpingitis.—In unilateral salpingitis it is not necessary to remove both tubes; the diseased one alone should be taken away. If the corresponding ovary be healthy it should be saved, but if infected with pus it must be removed with the tube. At the same time the uterus should be curetted to remedy the endometritis and prevent the other tube from becoming infected.

Double Salpingitis.—If a patient suffer from double salpingitis calling for operation—especially if there be a double pyosalpinx—I would strongly urge the removal, not only of the tubes, but also of the uterus. This does not add to the risk of the operation, and it makes the cure more certain. In these cases the uterus is deeply infected, and if left behind is not only a useless but a diseased organ. The patient often continues to suffer severe pain, to menstruate profusely, and to have irregular floodings. When not losing, she has a nasty purulent discharge. In several cases where I had removed the tubes alone for pyosalpinx I have had afterwards to do hysterectomy. If at the primary operation the entire uterus be removed, it is easy to drain the pelvis through the open vagina with iodoform gauze. One ovary should if possible be saved in order to continue the ovarian secretion. There is, as far as I know, no internal secretion of the uterus; at any rate, the removal of the uterus causes no marked disturbance of the woman's nervous system if some ovarian tissue be left behind.

Acute Pyosalpinx.—In acute pyosalpinx it is best to open and drain the distended tube by an incision through the posterior fornix. This allows the virulent pus to escape through a dependent opening, and there is little risk of infecting the general peritoneum. At a later date, if she be not cured, the abdomen may be opened from above, and the diseased organs removed with comparative safety. In years past I lost a number of cases of acute pyosalpinx by doing a primary abdominal section. The rotten tubes burst during the operation, the peritoneum became fouled with septic pus, and the women died of acute peritonitis. I am confident I should have saved nearly all these cases if I had first drained the tubes vaginally, and at a later date removed them from above.

Mortality of the Various Operations.—After all, the most important result of an operation is the result to the patient's life. The death-rate after different operations varies considerably. Let me illustrate this by a few figures taken from the annual reports of my own hospital, the Birmingham Hospital for Women. During the past ten years there have been performed altogether 3,385 abdominal sections, with 80 deaths (2.3 per cent.).

The mortality of operations for inflammatory diseases of the appendages was as follows:

	Cases.	Deaths.	Per Cent.
Removal of one ovary for chronic ovaritis	22	0	0
Removal of both ovaries for chronic ovaritis	2	0	0
Removal of one tube for salpingitis (including hydrosalpinx, haematosalpinx)	91	1	1.1
Removal of both tubes for salpingitis..	129	2	1.5
Removal of one tube for pyosalpinx ...	31	1	3.2
Removal of both tubes for pyosalpinx	134	6	4.4
Vaginal drainage of pyosalpinx and ovarian abscess	36	0	0
Removal of uterus and both tubes for salpingitis or pyosalpinx	147	2	1.3
Conservative operations on the appendages (including separation of adhesions, ignipuncture of ovaries, resection of ovaries)	392	2	0.5
	984	14	1.4

These figures represent the result of the work of all the members of the surgical staff of a modern gynaecological hospital during the past ten years. It will be observed that we operated on very few (24) cases of chronic ovaritis, whilst in a large number (392) conservative operations were performed on the appendages. As might be expected, the mortality of removing one tube was less than when both were taken away. In the case of pyosalpinx the mortality of vaginal drainage was nil (36 cases, no death); that of removal of one tube was 3 per cent., and that of removal of both tubes was 4.4 per cent.

Other Sequelae of Operations on the Appendages.

But if the patient does not die from the operation she is unfortunately not always cured. In certain cases there are after-complications which cause pain or discomfort to the patient and vexation to the surgeon. In addition to some already mentioned, let me briefly refer to the following:

1. *Hernia of the Scar.*—This is much less common since the abdominal drainage tube has been discarded and vaginal drainage employed when necessary. With few exceptions, a hernia of the abdominal scar is evidence of faulty technique on the part of the operator.

2. *Sinuses.*—These may be due to suppurating foci having been left behind in the abdomen or pelvis. They are rare if a clean sweep be made of the diseased organs. In some cases they are caused by the presence of septic foreign bodies—especially infected silk ligatures—in the abdominal cavity. Since I have given up the use of silk for ligatures and buried sutures, and have used iodine catgut exclusively, I have not been troubled with sinuses.

3. *Faecal Fistula.*—This is most common after operations for tuberculous pyosalpinx. It is apt to occur in those cases where the tube has to be stripped off adherent bowel, or where previous to the operation the pus sac has been discharging into the bowel. I know of nothing more distressing to a refined sensitive woman than this condition. The surgeon is often unjustly blamed for causing it, as it

is difficult to get the patient and her friends to understand that the fistula is due, not to the operation, but to the original disease.

4. *Uterine Discharges.*—I have already referred to the occurrence of menorrhagia after removal of the appendages for salpingitis. It may be so severe as to call for hysterectomy. It cannot, of course, occur if the uterus be removed with the tubes at the primary operation. Sometimes a very annoying purulent discharge from the uterus persists for months or years after removal of the appendages. It is due to an untreated endometritis, and may necessitate curetting or removal of the uterus to effect a cure.

Importance of Combining Medical and Surgical Treatment.—Some cases are easily remedied by medical treatment alone, and some are promptly cured by operation. But in many others both medical and surgical treatment must be combined to get a good result. For example, in cases of tuberculous pyosalpinx, we must not only remove the diseased organs by operations, we must also combat the general tuberculous condition by fresh air, dieting, tonics, and injections of tuberculin. In cases of gonorrhoeal salpingitis we may have to supplement our operations by the administration of iodide of mercury, urinary antiseptics, and gonococcic vaccines. In short, the modern gynaecologist must be both a physician and a surgeon.

DISCUSSION.

Dr. F. W. N. Haultain (Edinburgh) said he had not heard the entire papers, but must join issue with Mr. Martin in his sweeping statement that removal of the ovaries for epilepsy was unjustifiable. He had removed the ovaries in 5 cases of this condition, in 3 with complete success, in 1 with marked improvement, and in 1 failure. All of these cases showed marked association of the fits with the menstrual function, usually commencing with the onset at puberty, and at first being mainly confined to these times, but gradually increasing in severity and frequency. Under the same category other reflex conditions such as vomiting and asthma with enlarged and tender ovaries might be benefited by removal. In one typical case he removed one diseased ovary in a woman with asthma with completely good results for five years, when the other ovary became similarly diseased, with recurrence of the asthma. Since the removal of the second ovary she was completely and permanently cured.

Mrs. Scharlieb, M.D. (London), said: Cases of inflammatory disease of the uterine appendages should be first treated on medical principles, such as rest, diet, care of the bowels, and, if necessary, with serum and vaccine-therapy. These measures might be followed, if necessary, by curetting and painting the endometrium with iodized phenol. The next step in cases of pyosalpinx was incision through the posterior vaginal roof. In cases where abdominal section was performed for the removal of pyosalpinges or tubo-ovarian abscesses it was very desirable to secure free drainage by incision through Douglas's fossa. In many cases the adhesions behind the uterus were so dense as to make this incision difficult or impossible. If this were the case, the uterus should be removed. The uterus was the sac designed for the accommodation of the fetus until it was mature and fit for independent existence; it was not essential to the woman's health and well-being. The sacrifice of the uterus was of little moment as compared with the sacrifice of the ovaries. The ovaries should be spared whenever possible; if not both, then one; if not a whole ovary, at least a portion, for they were of great importance to the psychical and physical health of the woman. Abdominal section for suppurative disease of the uterine appendages was one of the most difficult and dangerous of gynaecological operations—really more so than Wertheim's operation for cancer of the uterus; but it was made less difficult, less dangerous, and more useful by the removal of the uterus.

Dr. Amand Routh (London) agreed almost entirely with the medical and surgical treatment of acute and chronic salpingitis as stated by Dr. Griffith and Mr. Martin. He referred especially to three methods of medical treatment: (1) The importance of sterilizing the vulva, vagina, and endometrium by appropriate methods as a preliminary to every type of surgical treatment of inflamed tubes, whether removed by the abdomen or drained by the vagina. (2) The vaccine treatment of gonorrhoeal salpingitis had, in his hands, proved very useful, and even in acute cases had led to the immediate cessation of the acute symptoms. If gonococci were found to be present a stock vaccine should be at once used, and if this failed, an autogenous vaccine should be injected. (3) Electric heat applied by electric lights swung on the limbs of a bed cradle for twenty minutes once or twice a day in subacute cases encouraged absorption and greatly hastened the cure of a perimetritis. In regard to operative treatment, he looked upon vaginal drainage of a pyosalpinx which could be reached by the vagina as the safest and best treatment. He regarded this treatment not merely as a preliminary to a subsequent abdominal operation, but, as in many cases, absolutely curative. He had known many cases where a complete cure had been thus obtained, and he had even seen pregnancy occur eighteen months after both tubes with purulent contents had been drained per vaginam. In all cases physiological rest should be insisted upon and the husband should be examined by an expert lest a chronic gleet were present. He considered the removal of a pyosalpinx during the acute stage to be one of the most dangerous of all abdominal operations, owing to the fact that almost all such pus tubes were the result of a mixed injection—usually gonococcal and streptococcal. Bumm had stated that the rupture during laparotomy of an acute pyosalpinx infected only by gonococci would not cause peritonitis; but, as most cases were due to mixed infection, it was far better to wait till the acute symptoms had subsided and the pus had become sterilized. He believed this discussion would prove how advisable it was to avoid abdominal operations during the acute stages of salpingitis. These cases rarely, if ever, died of their disease, and should be treated by palliative treatment or by vaginal drainage; abdominal operations should be left to those rare cases where they were subsequently indicated. In general, he cordially agreed with the modern as distinguished from the old-time Birmingham view.

Sir John Byers (Belfast) spoke of the value of the excellent introductory papers as indicating that, in the treatment of inflammatory diseases of the uterine appendages, they had now reached a state of stable equilibrium showing the value of both medical and surgical treatment. He emphasized the value of vaginal incision and drainage in acute collections of pus in the tubes, and also of vaccine-therapy. He specially drew attention to the prevention of inflammatory disease of the tubes and ovaries, which he thought was the great lesson of the discussion.

Professor J. B. Hellier (Leeds) said he was struck with the general consensus of opinion amongst gynaecologists to-day on the question of operative treatment of diseased appendages. He thought gynaecologists as a class were now cured of the "oöphorectomania" which was epidemic thirty years ago. Time and patience would often make the knife unnecessary, but many hospital patients who had to earn their own living could not or would not afford the time, and hence one had to operate. His experience of yeast suppositories for acute gonococcal vaginitis had been very satisfactory, and he used them also for preparing patients for vaginal operations. Posterior colpotomy for pelvic abscess was very good, but sometimes abdominal drainage would succeed when vaginal drainage had failed. He quite agreed that when both tubes were removed for infection it was often excellent practice to remove the uterus also.

Dr. H. Beckwith-Whitehouse (Birmingham) observed that from pathological reasons, in the majority of cases of chronic pyosalpinx it appeared justifiable to conserve the uterus. He had submitted to bacteriological examination a considerable number of uteri removed with double pyosalpinx, and the large majority he had found to be sterile. This observation coincided with the fact generally accepted, that the pus in chronic pyosalpinx was usually sterile. On the other hand, the endometrium of such uteri was usually oedematous and hypertrophic, and appeared to be a suitable soil for further infection. He

had found that a very satisfactory method of ascertaining whether organisms were still actively present in the uterus was to collect the menstrual blood and submit it to a careful bacteriological examination. If organisms were still present hysterectomy appeared to be associated with some risk, and removal of the appendages only should be attempted. He was glad to hear that conservative measures were advocated, and he was thoroughly in accord with the views so expressed. It was astonishing how pelvic adhesions disappeared. He recalled several cases in which a second laparotomy, performed some months after the first, had shown almost complete disappearance of adhesions. For this reason it was frequently advisable to divide operations for pelvic inflammation into two stages. The first should be simple incision and drainage of pus-containing cavities; the second, if required, was a radical operation performed some weeks later. With regard to conservative measures, the question of autoplastic ovarian grafts certainly had a place. In cases of double pyosalpinx with very adherent ovaries it was frequently possible to save the uterus and to graft minute fragments of ovarian tissue into a rectus muscle. He had performed this operation upon two occasions with distinctly good results. Menstruation had continued, and the patients were now in excellent health.

Dr. C. E. Pubslow (Birmingham) entirely agreed with Mr. Martin's advice that acute pus collections in the pelvis were in many cases most safely treated by vaginal incision, followed later, if necessary, by abdominal section; these cases were characterized by acute pyrexial symptoms and a mass, usually in Douglas's pouch; some of them were completely cured by the vaginal incision. He thought that when an acute salpingitis was due to gonorrhoea there was much greater probability of its spontaneous cure than when it was due to sepsis, and some of these cases, when acute peritonitis was present and the symptoms were severe, recovered in a surprisingly short time.

Dr. F. Edge (Wolverhampton) recalled that the late Professor Taylor drew attention to the common occurrence of gummata in the interstitial portion of the tube causing pyosalpinx. The free use of red mercury iodide usually cured these cases. With regard to pyosalpinx he (the speaker) had had many cases in former years where he had removed first one tube and later the other, still later the uterus subtotally, and later again the cervix. There was, therefore, a logical indication for total hysterectomy with removal of both appendages. A piece of ovary, if uninfected, might be left behind, but the patient must be told that the possibilities were ovarian pain from cystic formation and inclusion in dense adhesions. With working women who had to keep themselves, he advocated total removal after full explanation. It was only in this way, also, that one could avoid the tragedy of those specially virulent cases where conservative surgery with insufficient drainage led to death. The gonococcus cases were the most favourable for interference, while infections from the bowel or vermiform appendix were very dangerous. Even when a radical operation had been done there still remained sources of infection. He asked whether vaginal opening of pus sacs as a preliminary operation might not sometimes lead to mixed infection endangering the subsequent operation for removal of the diseased parts. With the general trend of the papers and discussion he was in complete accord. Vaccines and other new means might yet render surgical aid exceptional, but by that time prevention might eliminate even the need for vaccines.

Miss F. Ivens, M.S.Lond. (Liverpool), thought that future treatment would be an association of medical and surgical measures, and that the tendency would be to use vaccines in combination with conservative surgical treatment. After separation of adhesions and the opening of tubes vaccine treatment was of great prophylactic value in preventing the formation of fresh adhesions. She operated almost uniformly by the abdominal route, with carefully packed-off intestines; it gave good access and most satisfactory results, even in acute purulent cases. She used no drainage in gonococcal cases, but if a *B. coli* infection were present in addition, abdominal drainage was used in preference to vaginal. With regard to tuberculous tubes, in an experience of 20 cases she had only found it necessary to use tuberculin in 2 cases where tuberculous peritonitis had occurred later. These cases had been treated conservatively with removal only of affected areas, an effort being made to leave the whole or part of an ovary. The uterus was not removed, except in one case where a necrobiotic fibroid was present. No drainage was used unless a secondary bowel infection were present.

Dr. J. E. Gemmell (Liverpool) dwelt upon the degree of infectivity of gonococcal and mixed infections. He referred to a series of cases under observation and treatment some twenty years ago where clinical evidence established what had now been proved bacteriologically. Conservative treatment of chronic inflammatory appendage disease—opening of closed tubes, separation of adhesions—had not been very satisfactory; in no case had pregnancy supervened, and several cases had required a later operation. Early recognition of the infective nature of these diseases, active medical treatment, and a wider promulgation of the view that there was a risk to women of latent gonococcal infections, would do much to diminish chronic inflammatory appendage trouble.

Dr. E. Napier Burnett (Newcastle-on-Tyne) referred to the great difficulty which the gynaecologist experienced in deciding for or against operation in cases of infection of uterine appendages. Every one had experience of cases of pelvic cellulitis readily yielding to palliative treatment, while other apparently similar cases of pathological lesion defied all such medical treatment. The difference in such results was probably due to the varying degrees of personal resistance on the one hand, and, on the other hand, to the difference in the virulence of the infecting agent. He would like the openers of the discussion to state their experience of tubal pregnancies following the treatment of tubal inflammation on conservative lines; he believed this to be an important point for consideration. Regarding tuberculous salpingitis, he strongly condemned vaginal drainage in such cases, as it almost invariably led to mixed infection, with disaster to the patient.

Dr. Griffith, in reply, took the opportunity of discussing the more surgical aspects of the subject. He agreed with most that had been said as to the advisability of a rational conservatism. He noted that in the discussion surgery had dominated medicine. It appeared an obvious duty to save everything that could be saved. Tubercle was different from all other infections, and, if local, was extremely curable. He considered mercury a most useful drug, quite apart from its specific action on syphilis. Drainage, in his opinion, was always carried much too far. Acute cases undoubtedly needed it, but in most others it was quite unnecessary. Gonococcal peritonitis, if pure, was practically never fatal, although the patient might for a time look extremely ill. He regarded mere statements of the advantage of removing the ovaries in epilepsy and dysmenorrhoea as harmful; all such cases should be very fully reported.

Mr. Christopher Martin, in reply, stated that with reference to the question of the removal of the ovaries for epilepsy, his experience was quite different from that of Dr. Haultain; he thought that they should be very chary in recommending the operation in such cases. He was glad that so many speakers agreed as to the value of the vaginal drainage of acute pyosalpinx, the importance of saving one ovary when possible, and the wisdom of removing the uterus with the tubes in cases of double salpingitis. He was surprised to hear that Dr. Beckwith-Whitehouse found the uterus sterile in his cases of salpingitis; but even so, it was still a chronically inflamed organ and should be removed with the tubes. He agreed with Dr. Pursłow that cases of acute gonorrhoeal salpingitis were more likely to recover spontaneously than those due to sepsis or mixed infection. He was quite at one with Dr. Edge as to the great value of a course of mercury iodide in cases of salpingitis, as was first pointed out by his colleague, the late Mr. J. W. Taylor, but he could not agree with him as to the frequency of gummata in the uterus in these cases; what he called gummata were probably caseous abscesses in the uterine wall, and not of syphilitic origin. It was not usually difficult to diagnose pus in cases of inflammatory trouble; in addition to the

constitutional evidences of suppuration, a fixed mass was to be felt in the pelvis, behind or to one side of the uterus, with signs of thick fluid in it; if fluctuation could not be made out per vaginam it could often be detected on rectal examination; in some cases there was a dense, hard mass of effusion with a soft centre—the focus of suppuration. He could not agree with Miss Ivens that vaginal drainage for pyosalpinx was more dangerous than abdominal drainage; in his experience it was quite the reverse. He did not condemn the removal of cirrhotic ovaries absolutely, but thought that the operation should be done only in intractable cases after milder measures had failed. He was glad that Miss Ivens agreed with him as to the importance of combining medical with surgical treatment. In reply to Dr. Gemmell, it was true that patients might object to undergoing two operations where one would suffice, but if it were explained to them that it meant much less risk, they readily gave way. Several references had been made to the feud in years gone by between London and Birmingham. He was glad that this was now a thing of the past, that the hatchets had been buried, and that good feeling prevailed. In these battles his old master, Mr. Lawson Tait, was the chief fighter, and much of the animosity was due to the vehemence of his onslaughts on those who opposed him. In spite of the mistakes he made he was a great genius, and modern gynaecology owed much to his pioneer work. Let us forgive and forget his errors, and with gratitude remember his marvellous achievements.

CHORIO-ANGIOMA OF THE PLACENTA.

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TUMOURS of the placenta are of interest on account not only of their extreme rarity, but also of their structure and origin. I therefore felt that the members of the Association would be interested to see the specimen which I am able to show to-day. So far as I have been able to discover, this is the seventy-first case of a genuine tumour of the placenta (excluding cysts) that has been recorded. The actual incidence of the tumours is, however, probably not so rare, for one or two observers have recorded more than one, and all the cases have been discovered in hospital or institutional practice. In the *Transactions of the Edinburgh Obstetrical Society* I have found no record of any such tumour having been shown; and in the *Transactions of the London Obstetrical Society* only two—one by Galabin in 1885, and one in the beginning of this year by Drummond Maxwell.[8] Strangely enough, I had the opportunity of seeing still another specimen this summer, which has, I believe, been shown to the Pathological Society at their recent meeting at Newcastle by Dr. Stuart McDonald. It is interesting, too, to recall the fact that the first observation on the subject was made by an English physician, John Clarke, in the *Philosophical Transactions*, London, in 1795.

The patient from whom this specimen was obtained entered the Royal Maternity Hospital, Edinburgh, in December, 1911, during Professor Sir Halliday Croom's service, and I gladly take this opportunity to acknowledge his kindness in giving me freedom to examine and describe the specimen.

The patient was a woman of 25, who had had two normal labours previously and no miscarriages. The last menstruation had been in April, 1911. She complained of never having felt quite well during this last pregnancy, and in the last two months the abdomen increased rapidly in size. Latterly it caused some difficulty in breathing, and for a fortnight she had had considerable dyspnoea, which prevented her from sleeping. For some weeks she had felt a vague pain all over the abdomen.

On examination the abdomen was found distended beyond the size of a full-term pregnancy, and a network of distended veins was noticed coursing over the surface. The patient was suffering from marked dyspnoea. The pulse-rate was 96, regular but weak. The os uteri admitted one finger.

The patient was put to bed, the intention being to induce labour the following morning. At 10.30 p.m., however, pains began spontaneously and continued. The dilatation was slow, and the patient's distress considerable, so the membranes were ruptured above the fetal head, and 6 pints of fluid allowed to escape. After this the labour proceeded normally, and at 3.10 a.m. the child was born. It was obviously premature, and only survived twenty hours. The placenta required to be expressed. The patient's recovery was uneventful.

The Placenta measures 6 in. by 7 in., and the greater part of it is quite normal in appearance.

Maternal Surface.—At one side of the placenta there is a mass of smooth greasy-looking lobules of various sizes packed together into a fairly compact tumour measuring 3 in. by 4½ in. Repeated examination of the tumour has loosened the arrangement of the component lobules, which may be seen to vary in size from that of a large pea to that of a plum. Some of the lobules on the extreme edge of the tumour were detached by a mere touch. Most of them are attached to fine pedicles composed of one or two delicate vessels bound together in a sheath. The arrangement of several groups of lobules was very like that of a bunch of grapes on a stalk (Fig. 1). The uterine surface of each lobule has a greasy gelatinous appearance, most marked at the edges, where the lobules are moulded upon each other, and faceted like a collection of gall stones. Several lobules bear the impression of the vascular pedicle as a distinct gutter mark running over one or more surfaces.

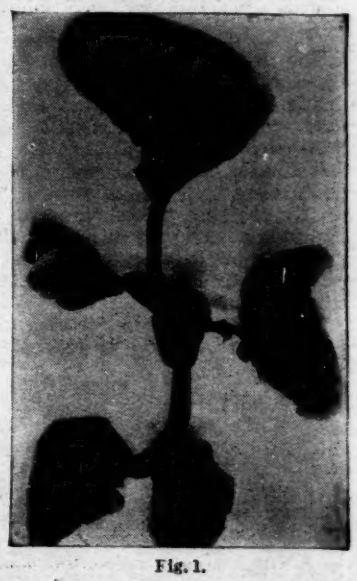

Fig. 1.

Fetal Surface (Fig. 2).—The cord is inserted eccentrically towards the opposite end of the placenta. It is thick and apparently normal. The tumour is seen to lie in a crescentic bay in the edge of the normal placental tissue, its surface being raised above the level of the normal part, and separated from it by a definite furrow. The tumour has the same greasy appearance as on the uterine surface. One or two large vessels run direct from the base of the cord to the tumour, over which they ramify, sending branches at right angles down towards the uterine surface—to which branches the lobules are attached as described. At the base of the cord these vessels are exceptionally thick, one being as thick as the little finger. This vessel shows a very decided constriction just at the base of the cord. The amnion passes normally over the entire surface of the placenta and strips readily from the chorion.

Fig. 2.—The tumour is situated at the lower left hand edge of the placenta. Note the vessels passing direct to it, and the definite constriction of one large vessel near the insertion of the cord.

The tumour appears to be separated from the normal portion of the placenta by a connective tissue capsule, but microscopic examination reveals that this is not so. On section the individual lobules show a gelatinous appearance at the edges and superficially. The deeper parts look quite homogeneous in structure and are dark crimson in colour.

The entire placenta was hardened in 5 per cent. formalin solution and sections cut from various parts of the tumour, placenta, and cord. Examination showed that the various lobules were all identical in the essentials of their structure.

Microscopic Examination.

The *covering* of each lobule consists of chorionic epithelium (Fig. 3). Both layers can be detected at many points. The most persistent appearance is that of large, more or less regular, rounded cells with large oval or polygonal nuclei. These are the representatives of Langhans's layer. Outside these a plasmodial layer is every here and there visible with smaller and more deeply staining nuclei grouped together. This is the syncytium. The thickness of the covering varies considerably, but it is never lost. At some points it is quite exuberant, at others only a thin film of syncytium is to be seen.

Where the vessels forming the pedicles enter the lobules, the covering is reflected over them, enclosing

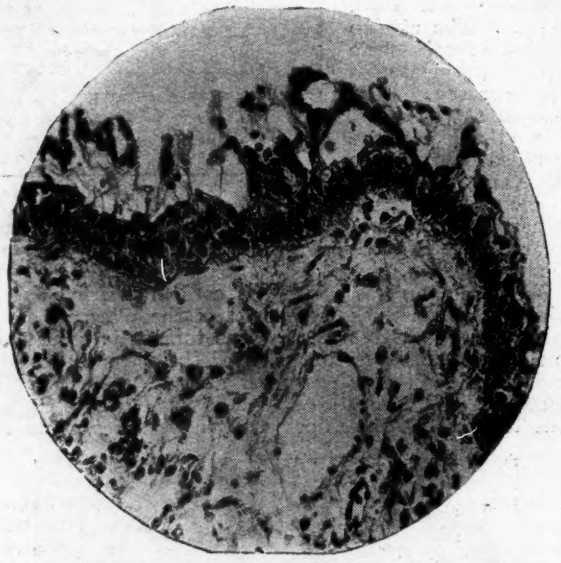

Fig. 3.—At this point the chorionic epithelial covering is particularly exuberant.

them in a sheath. The lobules lying in direct contact with the chorionic membrane have no pedicle, their structure being directly continuous with the connective tissue layer of that membrane. Their other surfaces are, however, covered with the two layers of epithelium like the rest.

Substance of the Lobules.—Immediately under the epithelial covering we find a layer, varying greatly in thickness at different parts, most marked at the edges and corners, and sometimes altogether absent, of very elementary and delicate connective tissue. The fibrils form a loose network, rather resembling a myxomatous tissue. It is this tissue that gives rise to the gelatinous appearance of the edges of the lobules and has led to many of the specimens being identified with myxomata.

Below this delicate layer lies the characteristic structure of the tumour. Briefly it consists of an enormous number of capillaries gorged with fetal blood, and supported by a fine connective tissue stroma. Here and there the vessels are large and have the ordinary structure of arteries and veins, but for the most part they are capillaries with walls consisting of a single layer of endothelium (Fig. 4). In many parts the capillaries are so fine, and so numerous, and so full of blood, that the appearance is very like that of a haemorrhage into the stroma. I believe, however, that there is no actual haemorrhage, for such areas pass gradually into others in which the capillary structure may be clearly seen, and even in the middle of these haemorrhage-like areas one can detect, amidst the jostling crowd of blood corpuscles, the dark elongated nuclei of the endothelial cells of the capillary walls.

Sections through the part where the tumour and the normal placenta are contiguous show that the villi next the tumour are compressed, and in many cases they show fibrinous degeneration. Here, as elsewhere, the tumour is covered with chorionic epithelium. There is, therefore, no proper capsule, but only a layer of flattened and degenerate villi and fibrin.

An interesting point is that at no point on the uterine surface of the tumour was there any decidua visible, either to the naked eye or under the microscope. In other words, there is no true "maternal" surface to the tumour, which had no intimate connexion with the uterine wall, and therefore took no part in the ordinary functional exchanges of the placenta.

With regard to the 70 cases of placental tumour that have been recorded, it may be observed in a word that the great majority of them correspond very closely to this specimen in their essential structure. It is true that they have been described under some dozen different titles, such as, "myxoma fibrosum chorii," "angioma placentae," "fibrosis chorii," "fibromyoma placentae," "chorioma placentae," and the like, but, according to Pitha,[1] who has gone very fully into this point, the essential structure in most cases is very much what has been described. The

Fig. 4.—Portion of a less congested area, with relatively few but large capillaries.

main feature is the presence of an enormous number of capillary vessels filled with blood. Further, careful study of the sections shows that one is dealing, not with an organizing haemorrhage, nor with a haemorrhage into a myxomatous tumour, but with an angioma, whose vessels are supported by a stroma, which in parts is myxomatous in type.

The question remains, From what structure does this angioma develop? I believe that the chorionic origin of these tumours may be taken as proved. Each lobule in the specimen before us is made up of exactly the same tissues as a chorionic villus—a covering of two layers of chorionic epithelium, delicate embryonic connective tissue and capillary vessels. Further, as was pointed out by Virchow, the stalked arrangement of the lobules at once suggests that the tumour arose in a bunch of early villi; while the fact that the pedicles are so thin indicates that the angiomatous lobules have developed in the terminal branches of the villi.

The cause of this angiomatous development has given rise to some discussion. It has been suggested that it was the result of some inflammatory condition of the deciduae. But the definitely localized nature of the tumour argues against that, or any other theory based upon *maternal* conditions.

There is some indication that interference with the fetal circulation is associated with the condition in the fact that almost invariably the tumour has a special blood

supply direct from the base of the cord. The typically congested condition of the tumour points to some obstruction in this special circulatory arrangement, but in all probability such obstruction is purely local, due perhaps to some kink, or constriction, or excessive tortuosity in the vessels. In the present specimen there was very decided constriction in a large vein passing direct from the tumour to the cord, with a considerable varicosity on the tumour side of it. This would certainly cause a distinct degree of venous stasis in the capillaries of the villi supplied by it, and it is quite possible that, if such an obstruction came into play at a very early stage of development, it might stimulate the capillary loops in the branches of the villi to this angiomatous overgrowth. We know, for example, that ligature of the hepatic vein may cause the development of an angioma in the liver.[9]

In some recorded cases the tumour has taken the form of a distinctly succenturiate mass, pointing to the probability that the circulatory disturbance has affected an entire primitive cotyledon of the placenta. The most probable theory would therefore appear to be that we are dealing with what is primarily an abnormal vascularization of a primitive cotyledon (or cotyledons), and that the occurrence of some obstruction in this abnormal vascular supply results in the enormous angiomatous overgrowth. This view places the origin of the tumour as far back as the third week of intrauterine life.

The literature of the subject shows that in many cases the presence of the tumour in no way affected the course of the pregnancy or labour. In others, as in the present case, there has been a degree of hydramnios leading to premature labour, with the subsequent death of the child. This association is interesting but not surprising, for we know that hydramnios is often due to interference with the fetal circulation. In several cases the placenta was adherent, but the abnormal adhesions appear to have been in the functionating part of the placenta. In itself the tumour appears to exert no influence directly on the pregnancy or labour, or upon the subsequent life and health of the child.

References.

[1] Pitha, *Ann. de gyn. et d'obstét.*, Paris, 1906, 2me série, tome iii, pp. 232, 269, 360; contains references to 62 cases. Other cases have been recorded by: [2] Ries, *Trans. Gyn. Soc. of Chicago*, 1904; 1 case. [3] Eggel, *Zentr. f. Gyn.*, 1905, No. 49, S. 1505; 1 case. [4] Buttermann, *Monatschr. f. Geb. u. Gyn.*, Bd. xxii, S. 591; 2 cases. [5] Bertoloni, *Ginecologia*, 1910, Heft 16; 1 case. [6] Kummer, Inaug. Dissert., Strassburg, 1911; *Zentr. f. Gyn.*, 1912, No. 1; 1 case. [7] Viana, *Monatschr. f. Geb. u. Gyn.*, Bd. xxii, S. 451; 1 case. [8] Maxwell, *Proc. Roy. Soc. of Med.*, London, 1912, vol. v, No. 4, p. 115. [9] Jores, *Virchow's Archiv*, Bd. clxi, Heft 3.

DISCUSSION.

Professor Chipman (Montreal) considered the case a very rare one; he had never seen such a condition. From the absence of decidual elements on the uterine aspect of this tumour, from its marginal position, and its extension beyond the attached margin of the placenta, it seemed to him probable that this tumour formation had occurred later in the embryonic cycle—at some time subsequent to the definite limitation of the placental area. In the excellent photomicrographs it was clearly demonstrated that the main mass of the tumour was made up of angiomatous capillaries; there were, nevertheless, areas of oedematous connective tissue reticulum, with few vessels sufficiently large and numerous to account for such tumours being classed as myxomatous; a thorough examination such as Dr. Johnstone had made would prevent such an error. He inquired whether in any of the 70 reported cases there were any observations made of vascular disturbances in the children. It was to be remembered that the fetal placenta was as much a part of the child as its arm or its leg, and that there was a tendency for vascular tumours to be multiple.

Dr. Johnstone, in reply to Dr. Chipman's remarks, said that in most cases the tumour developed at the edge of the placenta, where the villi of the chorion frondosum were presumably less attached to the decidua. In cases where the tumour was in the centre of the organ it was difficult to offer any explanation of the absence of decidua, unless it was that the unfixed villi—not the anchoring villi—were those most affected. In most recorded cases the children were born healthy, but in a few cases they were born prematurely as a result of hydramnios.

NOTES ON RUPTURE OF THE UTERUS IN CONNEXION WITH A CASE OF CONCEALED ACCIDENTAL HAEMORRHAGE.

By David Shannon, M.B.,

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In reading this paper I am actuated by the desire to learn the views of those better qualified to give an opinion on a subject about which comparatively little has been written—namely, severe accidental haemorrhage of the concealed variety, and more especially those cases in which tearing of the uterine wall takes place. Concerning these particular cases very little has been recorded. In nearly all our modern textbooks on obstetrics no mention is made of them, and in investigating the literature on the subject an article by Knauer seems to be one of the very few articles available for reference.

The case about which I wish to speak came under my care in the Glasgow Maternity Hospital.

She was a woman aged 40 years, a multipara, and was first seen by the house-surgeon on duty in the extern practice of the hospital. From her and from her friends he learnt that three weeks before she had been ill, but such illness could not have been of a serious nature as no medical man was asked to see her. Her friends told him that she was well, going about as usual performing her household duties, when suddenly she complained of great abdominal pain; she became faint and fell to the ground. The house-surgeon diagnosed concealed accidental haemorrhage and hurried her into hospital. I saw her on her arrival. She was very much collapsed and presented a typical picture of the disease from which she suffered. She complained of severe pain all over the abdomen. Her pulse was very rapid and her temperature subnormal; she was bathed in a cold, clammy sweat and was very pale. The clinical picture appeared to me to be one of shock rather than of haemorrhage. Palpation of the uterus was accompanied by great pain; no fetal parts could be felt as the uterine wall was extremely hard. The organ was rounder than normal and larger; it filled the whole abdomen. According to her own statement she was a little more than nine months pregnant. On auscultation no fetal heart could be heard. No blood was found in the vagina but a little serum escaped when the examination was made. The cervix was hard and would not permit of the entrance of a finger. The urine contained no albumen. The patient was anaesthetized and I performed Caesarean section and at the same time removed the uterus, but unfortunately she died one hour after the operation was completed.

When the abdomen was opened and the uterus laid bare I made out the following interesting facts, namely, the uterus was ruptured—small multiple incomplete tears. The peritoneal coat was the one mostly involved. There was a large tear at the junction of the bladder with the uterus measuring roughly between 2 and 3 in., and scattered here and there over the uterine surface were many smaller breaks in the continuity of the outer coat. Likewise, as I discovered after the operation, here and there in the muscular layer were small tears; into these tears blood was extravasated. As far as I could make out the mucous coat was not ruptured. A haematoma about the size of a small apple was found under the peritoneum on the left side of the uterus near the bladder. The wall of the uterus was very thin. Free blood was present in the abdominal cavity. Unfortunately the specimen was lost and the microscopic investigation was therefore rendered impossible.

In concealed accidental haemorrhage two factors have to be considered:

(1) The condition of the uterine wall; and
(2) The effects of the sudden increase of pressure caused by the blood that is rapidly poured into the uterus.

It is an accepted fact, I think, that all such uteri are in an abnormal state. Weiss mentions two cases in which suppurative metritis had been present, and probably in all these cases metritis exists. The fibrous tissue in the walls becomes increased more proportionately than the muscle, and in some cases so much so that the muscular elements become greatly diminished. In women who have had large families—the class in which concealed accidental haemorrhage is so common—it has been noted that the

increase of the fibrous elements in the walls of their uteri is very marked.

The placenta in these cases is also probably the seat of inflammatory changes, but I am inclined to think that in severe accidental haemorrhage the principal lesion is in the uterine wall.

Consider, then, for a moment such a uterus, and note the effects of greatly increased sudden pressure on its walls. A normal uterine wall will and does distend sufficiently to accommodate a full-term fetus, but is it not rational to think that a wall made up of fibrous tissue will not bear the strain and there must come a time when it will give way. When this accident happens the walls of blood sinuses will be lacerated and blood will be rapidly effused, causing a partial or complete separation of the placenta. Blood is poured rapidly and in great quantity into the space between the membrane and wall of uterus; its escape is rendered impossible owing to the membranes being adherent, or, what is more likely, to a sudden paralysis of the uterine wall from sudden over-distension. The blood collects in this space and increases the intrauterine pressure so much and so suddenly that the power of contraction of the muscular fibres is overcome for the time being and the uterus passes into a state of acute dilatation, and rupture follows.

Dr. A. W. Russell, in one of his cases in which no rupture was present, found the uterus in this dilated state, and recently Dr. Miles Philips in describing a similar case remarked on the marked thinness of the uterine wall.

From the point of view of sudden increase of fluid causing marked distension and giving rise to similar symptoms, though not nearly so severe, we have an excellent analogue in an ovarian cyst whose pedicle has become suddenly twisted. In such a cyst the vessels in the pedicle are twisted, the arteries less so than the veins, with the result that blood is poured into the tumour and none returns. Haemorrhage into the cyst takes place from the engorged vessels in its walls and raises the intracystic pressure so much that acute dilatation takes place and rupture may ensue.

Volvulus furnishes us with another analogous condition. In this diseased state the bowel becomes so much distended with gas that the peritoneal coat becomes fissured.

It is, I feel certain, this very marked and very sudden increase of intrauterine pressure that wrecks the uterus.

In external accidental haemorrhage no blood is retained in the uterus, therefore no distension take place and no urgent symptoms of shock make their appearance.

In the mixed variety blood is retained in the uterus, distension takes place varying with the amount of blood that is pent up, and symptoms midway between those of external and concealed are seen.

The peritoneal coat was the layer that was mostly involved. Here and there over its surface were small fissures, such as only could be caused by a general over-distension. They were most abundant in the regions of the fundus and posterior wall.

The tear at the junction of the bladder with the uterus measured between 2 in. and 3 in., and is of special interest in showing that this distension is the cause of the tearing. The bladder lies over the lower uterine segment, and its peritoneum stretches from its anterior surface on to the anterior surface of the uterus, to which it is firmly attached.

In cases of concealed accidental haemorrhage the lower uterine segment becomes, like the other parts of the uterus, markedly distended with blood and blood clot, but its walls being thinner, and on that account offering less resistance, the distension will be greater, and the peritoneum at this part will be so stretched that it will tear. This tearing would be more liable to occur if the bladder was full at the time the accident happened.

The haematoma found at the side of the uterus was formed probably by the rupture of some of the small vessels. It was about the size of a small apple and lay near the large peritoneal tear. At first I thought that the sudden formation of this haematoma might have been the causative agent in forming this large peritoneal rupture, but I feel certain now that it had nothing to do with its formation, as the peritoneum in the immediate vicinity of the haematoma was not ruptured. The muscular layer was thin. It did not contract, as a normal uterine wall would after the relief of pressure, but remained in a thin and flabby state, and on this account I am of the opinion that the whole uterus goes into a state of acute dilatation before rupture occurs.

The tears in this layer were small fissures, and were scattered over its surface. Blood was found in each of them, and its presence can be explained by the rupture of the small capillary vessels due to the excessive stretching. No large tears were present in the muscle. The mucous coat, as far as I could make out, was intact. It remains intact because it stretches so easily under pressure, and because the muscular layer behind it acts as a great support.

The amount of blood lost in this case was not sufficient to cause death. From the amount collected and from what was lost I am certain there was not more than two to two and a half pints. This has also been my experience in other cases of concealed accidental haemorrhage that I have seen. It has been said that a woman may lose five pints of blood and yet live, and I think it would be an impossibility for this amount to be suddenly effused into the space between the wall of the uterus and membranes when that organ was in its tenth month of pregnancy.

Excessive haemorrhage does not give rise to the symptoms one sees in a typical case of concealed accidental haemorrhage. I have never observed the same amount of shock in cases of severe external haemorrhage, in severe cases of placenta praevia, or in haemorrhage *post partum*, so therefore I infer there must be another factor present giving rise to this symptom. The shock, which is really the main symptom in these cases, is in all probability caused by the excessive pressure on and the stretching of the nerves in the wall of the uterus.

In other cases where the nerves of the uterine wall become disturbed by pressure, as, for example, in acute inversion of the puerperal uterus, symptoms of shock become prominent. Similar symptoms are also present in cases in which a hollow viscus ruptures or becomes acutely dilated.

It is in this later category I would be inclined to place cases of severe accidental haemorrhage of the concealed variety. Shock is probably the chief cause of death, and not the haemorrhage, although the more blood that is shed the greater will be the intrauterine pressure and the greater will the shock become.

Treatment.

The treatment of such cases is difficult, because, no matter what procedure is adopted, the majority of patients will die.

The first thing to be combated is the shock. If one believes that it is caused by this great and sudden increase of intrauterine pressure, then one must adopt measures to get rid of this pressure at once. My patient, when first seen, had 3 pints of saline injected into her veins, and I must say I think it did her more harm than good. It seems to me that such a procedure will tend to increase the amount of fluid in the uterus; by doing so the intrauterine pressure will become greater, and shock, instead of being diminished, will only be made worse.

With the idea in view of relieving this pressure the membranes should be ruptured as soon as possible and preparations made for instant operation in case profuse haemorrhage should supervene. After the membranes have been ruptured salines should be injected and strychnine or caffeine used liberally.

If the haemorrhage does not become profuse, the patient, now that the cause of the shock has been removed, should be allowed to lie still, in the hope that she may rally before any operation is performed. A major operation, with the patient in the midst of a severe shock, is in itself a very dangerous undertaking, but in some of those cases of severe accidental haemorrhage when a patient does not rally, or where, after rupture of the membranes, profuse haemorrhage makes its appearance, then we must operate at once so as to give the patient every chance.

The trend of thought amongst modern obstetricians is that in these severe cases Caesarean section followed by subtotal hysterectomy is the treatment that holds out most hope of cure. There are quite a number of cases now on record where lives have been saved by these procedures.

If it be a fact that in these severe cases of concealed accidental haemorrhage the uterus becomes acutely dilated or ruptured, then its removal is indicated, for if

it remains *post partum* haemorrhage will become so profuse that the patient will die. I have seen this happen in a case in which *accouchement forcé* had been performed and where the uterus had been plugged after the operation.

In one of my own cases—a case of mixed accidental haemorrhage—although the uterus was plugged after *accouchement forcé* had been performed, I had the greatest difficulty in checking a *post-partum* haemorrhage that nearly carried off the patient.

To accurately plug the uterus in these cases, where it remains a large flabby and non-contractile organ, is a difficult if not an impossible task. It was the fear of *post-partum* haemorrhage that made me adopt Caesarean section in this case.

DISCUSSION.

Dr. Grimsdale (Liverpool) discussed the question of salines intravenously in these cases, and observed that he considered that they were more likely to do good and less likely to do harm if administered after the bleeding vessels had been ligatured—that is, after hysterectomy.

Sir John Byrrs emphasized the difficulty of treating such cases. As a general principle he was not in favour of rupturing the membranes. He preferred Ringer's solution to simple saline as an intravenous injection, and was strongly of opinion that morphine was of great service.

Dr. Shannon, in reply stated that he ruptured the membranes in the case under discussion with the knowledge that the facilities for doing Caesarean section were at hand.

THE EXCRETION OF CREATIN IN PREGNANCY AND THE TOXAEMIAS OF PREGNANCY.

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The study of the excretion of creatin in pregnancy and the toxaemias of pregnancy has come into prominence during recent times and seems likely to lead to interesting results.

Whereas normally women do not excrete creatin, in pregnancy there is a considerable quantity of this substance to be found in the urine. How early in pregnancy this excretion starts is not known, but it is certain that by the third month considerable quantities of creatin may be found. In some animals pregnancy has the same effects; this was first shown in the case of the dog, and my friend, Dr. Edward Mellanby, has found this excretion a most useful test for pregnancy in rabbits prior to metabolism experiments. There can be little doubt also that this phenomenon may be useful in the diagnosis of pregnancy in the case of women.

Van Hoogenhuyze and Ten Doeschate have published the results of their investigations of the urine of pregnant women. They examined 8 cases of eclampsia, 2 of albuminuria of pregnancy, 3 of chronic nephritis with pregnancy, and 6 of normal pregnancy, and their results clearly show that in the abnormal cases the quantity of creatin excreted is very much greater than in the normal.

In the cases of eclampsia they found in almost every daily instance that 10 c.cm. of the urine contained more than 1 mg. of creatin, and in many as much as 4 mg. They obtained the same results in the cases of albuminuria and chronic nephritis. On the average they found about 2 mg. in all these abnormal cases. In normal pregnancy they found the excretion much less, and in almost all their estimations the reading was below 1 mg. In the course of a chemical investigation of eclamptic and toxaemic cases I had occasion to estimate the quantities of creatin excreted by such women, and although the cases examined were too few for conclusive decisions, my results were strikingly different from those of Van Hoogenhuyze and Ten Doeschate.

The cases investigated included both the normal and the abnormal. The normal cases were taken as a control, and in two of these the urine was examined daily for about a fortnight before and for the same length of time after delivery. In another it was examined a week before delivery and daily during the puerperium. In five other normal cases the urine was examined at intervals during the third, fourth, and fifth months of pregnancy. The estimations were done by Folin's colorimetric method, and as 10 c.cm. of urine were used in each case, the figures I give represent milligrams of creatin in 10 c.cm. of urine. In the cases at or near full term the daily figure varied between 1 and 3.5 during the fortnight before delivery. In one case the daily amounts in 10 c.cm. for the week before delivery were 2.5, 3.3, 1.6, 1.3, 1.0, 3.0, and 2.4. In another the average was about 1.5, and in the third the urine a week before the birth of the child contained 2.4 mg. in 10 c.cm. Soon after delivery, usually on the second or third day, there is a rapid increase in the creatin, and in these cases the averages for the week after delivery were 2.8, 3.7. and 2.6 respectively.

In the cases of early pregnancy examined I found creatin present in all, and the following figures represent the average amounts for these cases at the various periods of pregnancy:

At 3 months	= 1.1
At 3½ ,,	= 2.2
At 4 ,,	= 1.2
At 4½ ,,	= 1.8
At 5 ,,	= 2.5

Amongst the abnormal cases there were three in which there were numerous eclamptic fits; two in which the patients suffered from very severe toxaemia with large quantities of albumen in the urine, oedema of the hands, face, vulva, and legs, headache, vomiting, etc.; three patients whose urine became almost solid on boiling, but whose general condition gave rise to little anxiety (one of these had had eclampsia eighteen months previously, and her mother had suffered from this disease in each of her five pregnancies); one in which the albuminuria was due to chronic nephritis; and two in which the patients, both pregnant for the second time, were examined at about the seventh month because they had had eclampsia in their previous pregnancies.

In the eclamptic cases only one observation was possible before delivery in each case, and in two which were fatal only one afterwards. The third patient, whom my colleague Dr. Fairbairn delivered by Caesarean section, was observed daily until she left the hospital on the twenty-first day. In this case the amount of creatin excreted after delivery was larger than usual, but this increased excretion was due, certainly in part, to the abdominal operation.

In these cases the amounts of creatin excreted before and immediately after delivery were markedly less than in the normal cases. In one there was so little that the quantity could not be estimated, and in the other two the readings were 0.58 and 0.69 before and 0.46 and 0.92 respectively after delivery.

In the two severe toxaemia cases the results differed from one another.

In the first the urine was examined daily for forty-five days, twenty-four before and twenty-one after delivery. Before delivery the figure only rose on one occasion as high as 0.69, and remained low until the fifth day after delivery, when it rose, as in normal cases, to between 2.0 and 3.0.

In the second case the examination of the urine extended over three weeks, starting five days before delivery. During these five days the excretion of creatin was greater than in any other case observed, the daily readings being 8.3, 8.1, 7.3, 5.3, and 4.6. After delivery, however, the amount excreted rapidly fell to the level usual in normal cases.

The figures in this case before delivery were therefore in agreement with those obtained by Van Hoogenhuyze and Ten Doeschate. The remaining four patients—three albuminurias of pregnancy and one chronic nephritis—showed no variation from the normal creatin excretion, and the urines of the two patients who were examined on account of previous eclampsia were found to contain 2.0 mg. and 1.0 mg. of creatin in 10 c.cm.

Considered together, these results indicate not that there is an increased excretion of creatin in such pathological conditions, but that there is either a diminution from the normal, or sometimes even—in the worst eclamptic cases—complete suppression, at least at the time of the fits. These are the results, and it would be well to consider their meaning quite briefly.

Since Mellanby first emphasized the importance of the liver in creatin metabolism it has been clearly shown that the conditions in which creatin is most abundantly excreted are those in which the liver is under some harmful influence—for example, carcinoma of the liver (Mellanby), phosphorus poisoning (Leffmann), deficiency of or inability to use carbohydrates (Cathcart, Krause, and Cramer, Mendel and Rose, etc.), also toxaemias directly affecting the liver, such as general peritonitis and portal pyaemia (Mellanby), and operative interference affecting the portal area (Mellanby).

Consequently Van Hoogenhuyze and Ten Doeschate thought that they had good *a priori* reasons for expecting an increased excretion of creatin in eclampsia, for, as is well known, the liver in such cases when examined *post mortem* usually shows characteristic lesions. Yet in the cases which have come under my observation, when the *post-mortem* examination has revealed typical eclamptic livers, creatin was only present in the smallest detectable quantities.

My interpretation of these results is that the toxaemia of eclampsia is essentially different from the ordinary bacterial toxaemias with which we are familiar. Attention has recently been drawn to the similarity between the livers of eclamptic patients and the haemorrhagic livers of animals killed by anaphylactic shock.

Whether or no the study of anaphylaxis will ultimately teach us to understand the mode of action of the substances causing eclampsia and the toxaemias of pregnancy, it is clear that if the ordinary bacterial toxins acting directly on the liver cause an increased excretion of creatin, whereas the toxins of eclampsia acting on the liver suppress or diminish the excretion of creatin, then we have here a fundamental difference in their bio-chemical effects which seems worthy of further consideration.

RASHES OCCURRING DURING THE PUERPERIUM.

By Sir JOHN BYERS, M.A., M.D., M.A.O.,
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RASHES occurring during the puerperium are of great interest to the medical attendant as regards both their causation and diagnosis, while, when they assume a scarlatiniform appearance, they arouse the deepest dread and anxiety among the patients' friends. Let me give some examples of rashes I have observed during the puerperal period.

Iodoform Rash.

CASE I.—I was asked to see, in consultation with her medical attendant, a primipara on the fifth day after her confinement. Her temperature was 101° F., and her pulse 110. On the lower part of the abdomen, from the umbilicus to the vulva, there was a bright erythematous rash, which also involved the corresponding part of the back and the hips. The hands and face were swollen, but they were free from any rash. The perineum, which had been torn, was stitched, and had been dusted every night and morning with iodoform. No medicine of any kind—beyond a castor-oil aperient—had been taken. On ceasing the use of iodoform the rash disappeared, and the patient's fever and pulse fell, and she made a good recovery.

CASE II.—A multipara was admitted into the Belfast Maternity in the eighth month of her third pregnancy, owing to severe haemorrhage depending upon a central placenta praevia. The os admitting two fingers, the child was turned by Braxton Hicks's method, the foot was brought down and secured by a small piece of iodoform gauze. Labour set in after six hours, when the child was easily delivered. On the next day a rash similar to that seen in Case I appeared, but there was no fever and no rise of pulse. The eruption remained for twenty-four hours.

I have seen three other cases of iodoform rash during the puerperium (that is five in all), but now that much simpler methods are used than formerly, when powdered iodoform was often applied in the case of a sutured perineum, such rashes are not so common. The diagnosis in these cases depends upon a red, often itching, rash occurring after the use of iodoform, in the form of powder, either to a wounded perineum, or used as a fillet, or packing,—(an erythema toxicum)—and disappearing when the drug is no longer brought in contact with the patient. After ceasing the use of iodoform in these cases, the rash, as a rule, disappears within twenty-four hours. In some patients, with great susceptibility to the drug, the hands, arms, and face will swell, and occasionally constitutional symptoms (fever, headache, sickness, and gastric irritability) will be present at the time of the appearance of the eruption. In two of the cases I saw in consultation, the friends of the patients were in great terror from the believe that scarlatina had developed.

Lysol Rash.

I have seen several cases in which the use of a lysol lotion (a teaspoonful to the pint) employed to bathe the vulva in the puerperal period has caused a local irritation with a red, itching rash, which has spread from the perineum upwards to the abdomen and backwards over the sacrum, followed by a slight desquamation. Usually the rash and irritation cease on using some simple lotion, such as boracic, instead of the lysol, but in one case the irritation of the vulva and perineum continued long after the employment of lysol had been given up. I may say that, personally, I cannot use lysol, as it causes on my own hands and arms an itching whealing of the skin, due probably to idiosyncrasy.

Urticaria Rash.

In some patients, not at other times subject to urticaria, this rash appears during the puerperium, irrespective of any external (bites of insects, lotions) or internal (articles of food) causes. It occurs often in women who are not of a neurotic temperament, and may in such cases be associated with the functional activity of the uterus. I have attended a patient who in each of her three confinements has developed this rash. It appears on the third or fourth day after delivery on the abdomen, chest, and limbs in the form of wheals which came out in crops for a couple of days, and are attended with intense itching and burning of the skin. There is slight fever (100° F.) and quickness of pulse (90 to 100), which continue while the rash is out. The least scratching of the skin developes new wheals, which, when the skin is stretched temporarily, disappear. Curiously this patient to whom I have referred never has an attack of urticaria except during the period immediately after childbirth.

Sudamina or Miliaria Rash.

In some patients, after delivery, vesicles appear over the front of the chest and abdomen, like, as Jamieson puts it, drops of dew upon the skin, due obviously to obstruction of the sweat glands, but occasionally—especially under the binder—a red inflammatory condition develops both in and around the ducts, and the fluid in the vesicles becomes of a serous nature. As a rule, although locally there is itching, no constitutional symptoms arise. On the other hand, when sudamina are observed with an accompanying bright red rash (usually of a septic nature), there are marked signs of constitutional disturbance, and the prognosis is, in my experience, bad in such cases. I have seen several cases of both of these types of sudamina.

Rash of Erysipelas.

Since the rigid application of the aseptic and antiseptic methods to obstetric practice, cases of erysipelas are now much rarer than formerly, but during the past two years I have seen four cases. In two—both primiparae—the perineum was torn (one before she entered the Belfast Maternity Hospital, in an attempt at delivery in a slightly contracted pelvis), and in both the rash began at the seat of the tear and spread up the abdomen and down the thighs with great constitutional disturbance. The eruption was red, not elevated; there was some accompanying oedema, and vesicles appeared in one of the cases, and in both a slight desquamation followed. A house-surgeon and a student looking after these cases had each a septic finger. These were clearly cases of puerperal septic infection with an external erysipelatous rash. Fortunately they both, after a serious illness, recovered. In another case, four days after delivery an erysipelatous, rash developed on the face, beginning at the edge of the right nostril; while in the fourth patient, a week following childbirth, a well-marked attack of erysipelas showed itself at the left nipple and gradually spread over the chest. In both of these latter cases, the constitutional symptoms were slight, and there was no interference with the gradual involution of the pelvic organs. While modern bacteriology has taught us that the streptococcus of erysipelas and the streptococcus most commonly associated with puerperal infection are practically identical, it is an interesting

fact, needing scientific explanation, that when erysipelas begins in the face, scalp, or breast of a puerperal woman, the progress is much more favourable and the constitutional symptoms are much less severe than when the rash appears first at the genitals, probably owing to the lowered resistance of torn or bruised tissues. In his monograph on this question Winckel[1] gives an account of 42 cases of erysipelas occurring during pregnancy and the puerperium, of which 36 developed after delivery, while only 6 were seen during pregnancy. Of the 36 puerperal cases, 28 started from the vulva, and of these 12 died; of the remaining 14 cases, 6 during pregnancy (none of which originated in the genitals), and 8 during the puerperium, (2 commencing in the breast and 6 in the face and scalp) only 2 died.

Scarlatiniform Rashes.

Of the rashes resembling the eruption of scarlatina occurring in puerperal women, I have met with two types —one mild, the other severe.

(a) Mild Type.

CASE I.—I saw in consultation a primipara on the fifth day after her confinement. The labour, I was told, was a rather difficult forceps one, due to the head (a male child) remaining persistently occipito-posterior. The perineum was torn, and required four silkworm sutures. At the time of my visit the temperature was 103°, the pulse was 120, and the front of the abdomen was covered with a red petechial rash. There was slight tenderness over the uterus, and the lochia were somewhat foul-smelling. There was no sore throat, no enlargement of the submaxillary glands, and an absence of any history of exposure to the contagion of scarlatina. The friends of the patient were most uneasy, as they said a sister had died of scarlatina immediately after her confinement. I douched out the vagina with a weak creolin lotion, and introducing the gloved forefinger into the uterus, found it quite empty. I then took a swab from the cavity of the uterus, and gave an intrauterine douche. The head of the bed was raised, and a 5-grain dose of quinine administered. A stock vaccine (20 million streptococci) was injected. A subsequent bacteriological examination of the swab showed an infection of streptococci, and an autogenous vaccine was being prepared, when I heard from her attendant that the patient was better, her temperature having in twenty-four hours fallen to normal. There was no subsequent albuminuria and no case of scarlatina developed in her household.

I have seen five other cases with a similar rash, in two of which there was slight desquamation. They were all clearly cases of sepsis with a red rash, and, fortunately, of a mild type.

(b) Severe Type.

CASE II.—On October 14th, 1911, a patient was delivered in the Belfast Maternity of her first child. The labour was natural (first vertex). The patient was in good health, but it was noticed that the tongue had been cut by the stumps of some carious teeth. Two days after childbirth she complained in the evening of feeling chilly, and her temperature rose to 99.6° F., but fell next day to normal. On the evening of the third day her temperature rose to 103° F. and the pulse to 100, but she did not feel ill. The lochia were normal in quality and quantity, there was no abdominal tenderness, and, indeed, there were no physical signs to account for the pyrexia. On the morning of the fourth day of the puerperium, a bright petechial scarlet rash appeared on the upper part of the chest and the abdomen. It subsequently spread all over the body and limbs except the face. There was no angina of any kind, the submaxillary glands remained normal, and there was neither initial headache nor vomiting, and no subsequent albuminuria. For a couple of days she was delirious. The skin peeled (as it often does in such severe erythemata), but the desquamation on the hands was an early not a late feature. The patient was treated with two injections of a polyvalent antistreptococcic serum and cold sponging, and made an excellent recovery. When the rash was at its height it looked uncommonly like scarlatina, but there was a total absence of all the other symptoms of that fever. At this time there was a severe epidemic of scarlatina in Belfast, and I asked Dr. Gardner Robb (Medical Superintendent of the City Fever Hospital, Belfast, and Visiting Physician to the Union Fever Hospital, Belfast) to see her. He agreed with me that it was a severe septic case with a scarlatiniform rash, but not scarlatina. She remained in hospital until December 9th, 1911, and no other patient or person in the maternity during her residence developed either scarlatina or puerperal infection. She nursed her baby, which kept quite well.

I have seen three other cases with a similar widespread "scarlatiniform" rash, all fatal; and in none of the houses in which they were confined were there previously any cases of scarlatina, nor did any arise subsequently.

Relation of Scarlatina to the Puerperium.

These cases with a scarlatiniform rash raise the important question of the relation of scarlatina to the puerperium, about which there has been considerable discussion, and in relation to which a very extensive literature has arisen.

Has the puerperal woman a special liability or not to scarlatina?

Is scarlatina specially modified when the disease attacks a puerperal woman?

Is scarlatina—when it arises immediately before or after labour—a very serious disease with a high mortality? Or are the great majority of cases of so-called "puerperal scarlet fever" sepsis (puerperal infection) with a red rash resembling the eruption which is seen in true scarlatina?

The public have an intense dread of scarlatina occurring in a woman at the time of her confinement, and before the era of antiseptic and aseptic midwifery the same view was formerly held by many obstetricians. Olshausen[2] collected 134 cases with a 48 per cent. mortality; Dr. McClintock,[3] in 34 cases observed at the Rotunda Hospital during his Mastership, reported a 29.7 per cent. mortality; while Dr. Halahan, who practised in Dublin,[4] observed 25 private cases with a death-rate of 76 per cent. Dr. Braxton Hicks, in *A Contribution to our Knowledge of Puerperal Diseases*, reported that out of 89 puerperal cases with fever seen by him in consultation, 37 were due to scarlatina (of whom 27 died), although 17 had no rash, and almost none of these patients showed any throat lesion. Even at the present time some of the current textbooks of midwifery and medical treatises teach that puerperal women are specially prone to scarlatina, and that when such a complication occurs the death-rate is high. Others say that when scarlatina appears in the puerperium it is specially modified. Personally I believe that scarlatina is rarely met with during the puerperium, because women at that time seem, to a large extent, to be immune from the disease, either owing to the circumstance that they have had it before, or because of the immunity given by age, and that the so-called "puerperal scarlatina" (like most cases of "surgical scarlatina") is not really true scarlatina, but rather a form of puerperal infection with a red rash. In support of these views let me submit the following evidence.

1. *Personal Experience.*

I have never seen in private or consultation practice a case in which a woman after childbirth developed scarlatina. I can recall two cases after delivery which were thought to be scarlatina, but it was at a time before asepsis and antisepsis were regularly used in obstetric practice, and before bacteriology had become a science. On the other hand, I have observed a number of puerperal women with a rash which clinically could not be distinguished from that which accompanies true scarlatina, and which in the past would have been diagnosed, unquestionably, as true scarlatina. In all cases of women who after delivery develop a scarlatiniform eruption, puerperal infection should be suspected, and a bacteriological examination of the contents of the uterine cavity should be made, and treatment, especially by vaccines, should be begun, unless some of the other signs which make up the typical picture of scarlatina are present, such as the well-known appearance of the throat, enlargement of the submaxillary glands, the strawberry tongue, or the nephritis, as well as a history of exposure to contagion, and the spreading of the zymotic to those in direct contact with the patient. It is unfortunate that the *materies morbi*—which causes scarlatina—has not yet been isolated.

2. *Evidence from Maternity Hospitals.*

I have never seen scarlatina arise in a puerperal patient in the Belfast Maternity Hospital, and I find by inquiry that the experience in the maternity of the Union Infirmary in Belfast is the same. Among 16,000 puerperal patients in the Berlin Obstetric Clinic, A. Martin reports that there were only three cases of scarlatina. In his *Textbook of Obstetrics*, sixth edition, 1909, page 675, Dr. B. C. Hirst, Professor of Obstetrics in the University of Pennsylvania, states that in twenty years' hospital service in the Philadelphia Maternity and University Hospitals he has met but two cases of true scarlet fever in the puerperium. It is a remarkable fact that in the epidemics of scarlatina in the General Lying-in Hospital, London, recorded by Dr. R. Boxall,[5] in the Copenhagen

Maternity Hospital by Dr. Meyer,[6] and in the Queen Charlotte's Lying-in Hospital by Dr. Brown,[7] there was no mortality. Only about 1 per cent. of the lying-in patients during the epidemic in the Copenhagen Maternity acquired the disease, and Meyer points out that in the years 1871 to 1885 there were only 2 cases of scarlet fever in the lying-in period among the women confined in the Copenhagen Maternity, while in six years only 3 cases of the kind were noticed in the Infectious Diseases Hospital in Copenhagen.

3. *Evidence from Hospitals for Infectious Diseases.*

Dr. Gardner Robb tells me that during his lengthened experience of fifteen years in all at the Belfast City Fever Hospital and at the Union Fever Hospital, Belfast, he has only seen one case of scarlatina in a puerperal woman.

4. *The Effect of the Prevalence of an Epidemic of Scarlatina on the Puerperium.*

Scarlatina in an epidemic form commenced in Belfast in the middle of October, 1909, and continued throughout 1910 and 1911. In 1910 there were 734 cases notified, giving an attack-rate of 1.8 per 1,000 of the population; in 1911 the number notified was 766 (being 181 in excess of the average of the ten years, 1901–10). In other words, in the two years, 1910 and 1911, there were 1,500 cases of scarlatina notified, of which 1,259 were removed to the two fever hospitals. During the same years (1910–11) there were 2,188 deliveries at the Maternity Hospital, in the intern and extern departments, and 776 at the Belfast Union Hospital Maternity—that is, 2,964 deliveries in all; and yet at neither of these institutions was there a case of scarlatina, nor during these two years (1910–11), when there was such a widespread epidemic of scarlatina in Belfast, was a single case of that disease attacking a puerperal woman admitted into any of these maternities or into the fever hospitals of the city.

Conclusions.

From the above facts I think we may draw the following conclusions:

1. Scarlatina is rarely encountered during the puerperium.
2. When it does occur during the puerperal period it is, as a rule, a mild disease—that is, with the use of modern aseptic and antiseptic precautions.
3. What has been called "puerperal scarlatina" is almost always a form of septic infection with a rash resembling the eruption seen in true scarlatina.
4. From a person ill of scarlatina there may be conveyed to a lying-in woman either:
   (*a*) A true scarlatina; or
   (*b*) Septicaemia, arising from the secondary septic changes which are apt to take place in the throat, etc., in scarlatina.

The former (*a*) occurrence seems to be rare, while the latter (*b*) should be avoided by modern aseptic and antiseptic precautions.

REFERENCES.

[1] "Ueber das puerperale erysipel," Separat Abdruck aus dem "Aerztlichen Intelligenz-Blatt," München, 1885. [2] "Untersuchungen ueberdie Complication des Puerperium mit Scharlach und die sogenannte Scarlatina puerperalis," *Archiv für Gynäkologie*, 1876. Bd. ix, S. 169. [3] *Dublin Quart. Journ. Med.*, 1866. [4] Ibid., 1863. [5] *Obstetrical Transactions*, vol. xxx, 1888. [6] "Ueber Scharlach bei Wocherinnen," *Zeitschr. f. Geb. u. Gyn.*, xiv, 1888, p. 289. [7] BRITISH MEDICAL JOURNAL, February 8th, 1862.

DISCUSSION.

The PRESIDENT said the question raised was very important, and Sir John Byers seemed to him to have brought forward ample evidence of the need of being, if possible, doubly sure before making a diagnosis of scarlet fever in a lying-in woman.

Dr. BECKWITH-WHITEHOUSE (Birmingham) remarked that he was much interested in Sir John Byers's paper, particularly with regard to urticarial rashes. He had not seen such a condition follow labour, but he had a case at present under observation where the typical "wheals" developed shortly after each menstrual period. He had at first attributed it to the excretion of calcium in the menstrual blood, but no good had accrued from the administration of calcium. With regard to drug rashes, he thought that some mention should be made of iodine. Now that iodine was so largely used in the cleansing of the skin before operation, it might be of interest to note that hyperaemia and vesicular rashes had not infrequently followed the application of iodine, especially to the vulva, but also to the abdomen. This had followed even when no dressings had been applied. With regard to the scarlatiniform eruptions, he was quite in accord with Sir John Byers. Quite recently at the General Hospital, Birmingham, such a case had been admitted to Dr. Thomas Wilson's ward. Desquamation had occurred, and it was thought that the woman had undoubtedly suffered from scarlet fever before admission, yet no subsequent fever developed in the ward.

Dr. STOOKES (Liverpool) agreed with Sir John Byers on the rarity of scarlet fever during the puerperium. The records of the Liverpool Maternity Hospital showed it to be non-existent as a complication. At the same time, he had seen cases both of scarlet fever and erysipelas during the early part of the puerperium. One case especially, which, on the day after delivery, had a marked attack, ran a normal scarlet fever course. In the Liverpool City Hospital, Fazakerly, Dr. Rundle mixed cases of infectious disease and, with precautions, there was found to occur no spread of the diseases.

Mr. S. ENGLISH (Manchester) suggested that as most children had had scarlet fever before puberty that might account for its rarity during pregnancy and the puerperium. There was no reason to believe that there was any other cause than this for their immunity. It would be wise to inquire in a doubtful case if the patient had had scarlet fever.

Dr. J. P. HEDLEY (London) remarked that scarlet fever was a rare disease in adults; therefore one would expect it to be rare in the puerperium, especially as a woman late in pregnancy would avoid contact with scarlet fever patients. Sir John Byers's numbers from the Belfast fever and maternity hospitals were not strictly comparable, as those from the fever hospital presumably contained a large number of children. There was no reason why a puerperal woman should not develop scarlet fever.

Dr. PURSLOW (Birmingham) said he had had charge of an extern clinic in which some 400 to 500 cases were attended annually, and during twenty years had seen no case of scarlet fever among these patients. He thought that an argument in favour of Sir John Byers's view was that cases of scarlet fever during the puerperium were now generally considered to be rare, whereas years ago the association was considered to be quite common. In the meantime the general incidence of scarlet fever, so far as he knew, had very little diminished. The inference clearly was that many of the older cases described as scarlatina were undoubtedly instances of septic rash, and were rarer now owing to the adoption of antiseptic measures.

Sir WILLIAM SMYLY (Dublin) and Dr. FOTHERGILL (Manchester) generally endorsed Sir John Byers's conclusions.

Sir JOHN BYERS said his object had been to show that the teaching of the past, and even of some modern textbooks, that puerperal scarlatina was common, and that it was a serious obstetric complication with a high mortality, was wrong; and that, like "surgical scarlatina," it was the result of faulty observation, which more modern clinical scientific methods had corrected.

A NOTE ON SOME DEVELOPMENTAL ERRORS OF PUBERTY.

By W. E. FOTHERGILL, M.A., B.Sc., M.D.,
Manchester.

THE object of this note is not to bring out fresh observations or new theories, but to make a suggestion towards the improvement of our method of teaching errors in the development of the female reproductive organs. Most books give a little information on this subject under the heading "vulva," a little more under "vagina," more under "uterus," and so on. Thus the matter is dealt with

in snippets which are scattered throughout the volume. In a few books a much more satisfactory plan is used, and all the malformations are grouped together in one section or chapter. The suggestion now made is that this section or chapter ought to be divided into two portions. One of these should deal with errors of development which occur during embryonic and fetal life, such as doubling and atresia. The other should discuss errors which occur in the second developmental phase of the organs—namely, at puberty.

No one will deny that the female reproductive organs pass through these two phases of rapid evolution with a period of relative rest between them. It is clear that the organs may be formed normally *in utero*, but that the changes which occur at puberty may be abnormal.

There must, therefore, be developmental errors of puberty. The suggestion now made is that this should be recognized and taught. Some errors of this kind are doubtless truly congenital—due to some inherent defect in the ovum, while others may be produced by some elusive, extraneous cause acting during infancy or early childhood. The abnormality is generally on the side of under-development rather than on that of overgrowth; but there are cases of hypertrophy at puberty affecting, for example, the labia minora, the clitoris, and the breasts. Undergrowth is exemplified by various degrees of infantilism. Thus a girl may be a normal child until the time when the changes of puberty are expected. But she may then remain like a child for the rest of her life. In less marked cases the girl grows in size and her external appearance becomes more or less womanly, but the reproductive organs remain childish and do not function. In other cases, again, there are no physical signs of infantilism, but menstruation and ovulation are indefinitely delayed. There is no need to treat the many varieties of infantilism in detail, but two examples of maldevelopment at puberty may be briefly discussed.

Hypertrophy of the Cervix.

It is well known that the uterus of the child has a relatively large cervix and a small body. Indeed, the cervix often forms two-thirds of the whole organ. If the changes leading up to puberty are normal the fundus grows much faster than the cervix, so that when the development of the organ is complete the body of the uterus forms two-thirds of the organ and is twice as long as the cervix. But occasionally the cervix grows too fast at puberty and the body of the uterus fails to outgrow it. The result is an organ 4 in. or 5 in. long instead of 3 in. long. In the most striking instances the overgrowth affects mainly that part of the cervix which projects into the vagina, but in other cases that portion of the cervix which is above the vagina is also involved. Hitherto a somewhat artificial and over-accentuated distinction has been drawn between hypertrophy of the vaginal portion and hypertrophy of the supravaginal cervix.

Clinically this abnormality in development at puberty presents varied pictures. In the most pronounced cases the cervix grows down to the vaginal orifice, dilates it, and peeps out by the time the patient is 17 or 18 years of age. Ulceration and infection of excoriated surfaces may then occur, and the patient is brought to hospital complaining of "falling of the womb." The condition, however, is easily distinguished from true prolapse, because there is no cystocele. Both the anterior and the posterior vaginal fornices are present, though they may be shortened by inversion from above downwards of the upper part of the vagina. In such cases a portion of the cervix is amputated, the perineum, if very much stretched, is repaired, and the case is heard of no more.

In less marked cases the patient complains only after marriage; dyspareunia is the trouble, and the treatment is amputation of the cervix as before. Sometimes the patient notices nothing until she has become pregnant. Then the hyperaemia of pregnancy causes her to complain of "something coming down," and the hypertrophied cervix is found nearing the vaginal orifice. It has often been amputated during pregnancy without causing abortion. But, if this is not done, the elongated cervical canal softens, and opens out during labour, and seldom offers serious resistance to the passage of the child. It generally splits on one side or both, leaving lacerations which, if they do not become infected, do little harm. The puerperium over, the patient, who had a long cervix before pregnancy, has a long split cervix afterwards, and that is all. But, if involution is interfered with in any way, the cervix remains subinvoluted as well as the corpus uteri. The patient's cervix will then be larger than ever.

It is usual to describe a form of hypertrophy of the cervix in parous women, but this is unnecessary. Indeed, the distinction between hypertrophy in virgins and hypertrophy in parous women is a mistake. True or primary hypertrophy of the cervix is an error of development at puberty, and it may be exaggerated by incidents of parity. A woman who has a normal cervix as a virgin may of course have her cervix enlarged subsequently to parturition by oedema, congestion, and subinvolution. But such enlargement is not true hypertrophy of the cervix. It is merely enlargement secondary to other pathological conditions. The advantage of this way of regarding hypertrophy of the cervix is that it simplifies teaching. It does away with the distinction between hypertrophies of different parts of the cervix, and also with that between hypertrophy in virgins and the same in parous women. Thus one condition replaces four, and a mass of useless and artificial distinctions which the student used to be asked to memorize are done away with.

The Menorrhagia of Puberty.

The menorrhagia of puberty is another good example of an error in the developmental changes of puberty. The clinical picture is fairly familiar. A girl of 13, 14, or 15 begins to menstrate without pain, but the discharge is too profuse, and it continues for two or three weeks at a time. Anaemia is produced, and fainting is frequent. Sometimes with treatment, and sometimes without any, the lapse of months is followed by gradual improvement, and the periods finally become normal. On physical examination the uterus in these cases is found to be narrow, soft, and flabby; it is generally normal in length and may be longer than usual. The cervix is found to be open, and offers no resistance to dilatation. If the condition has lasted for some months, the endometrium is hypertrophied, and comes away in thick, fleshy flakes under the stroke of the curette. On microscopic examination the endometrium shows no variation from the normal structure; it is simply overgrown. The muscular wall of the uterus has not been submitted to microscopic examination by the writer.

But it is well known that, during the years before and after the appearance of the menstrual flow, there is a great increase in the uterine muscle relative to the uterine connective tissue, and this corresponds, in the normal individual, to the increase of blood supply which is observed as the reproductive organs begin their functional life.

Uterine haemorrhage is normally under the control of the muscular walls of the organ, and when this muscular control is absent bleeding is excessive. The most striking illustration of this is *post-partum* haemorrhage. Another is the menorrhagia of the menopause, which occurs when the uterine wall becomes demuscularized by age changes before its blood supply is proportionately reduced by physiological obliteration of the vessels.

Again, menorrhagia is observed when the tone of the involuntary muscle of the body is reduced by the toxins of various diseases, such as influenza, and when the muscle is weakened by malnutrition, as in certain cases of anaemia. The uterus at puberty, if its muscle does not develop properly, is in a state of chronic hyperaemia; and this causes the overgrowth of its endometrial lining which is clinically observed on curetting.

The view that this particular kind of menorrhagia is due to developmental muscular inadequacy is favoured by other clinical features. Thus the treatment of the anaemia does not lessen the bleeding in these cases, while in muscular weakness secondary to anaemia the administration of iron cures both the anaemia and the excessive menstruation. In the menorrhagia of puberty iron makes the bleeding worse. Again, the use of ergot, hydrastis, strychnine, and quinine generally checks uterine bleeding by acting on uterine muscle, but these drugs are of no use in the menorrhagia of puberty. This is understood if it is recognized that in this condition there is not enough muscle for drugs to act upon.

Two therapeutic measures only appear to be available in

these cases, namely, rest in bed and curetting. Girls in this condition progress most favourably if they are kept at rest with the foot of the bed raised throughout each period. No doubt this alone would suffice, but the endometrium must be restored to its ordinary thickness. The atrophic and degenerative changes in the endometrium which form a part of each menstrual cycle would no doubt bring about a normal condition in time, after the uterine muscle had developed sufficiently to prevent undue hyperaemia. But time is saved by scraping away the hypertrophied endometrium with a sharp curette. In several cases which have ultimately done well curetting has been repeated twice and three times.

DISCUSSION.

Dr. PURSLOW (Birmingham) said he had seen many cases of cervix protruding and ulcerated in young unmarried women, and could not agree that this was usually the result of congenital elongation of the cervix; on the contrary, he thought that these cases were nearly always due to prolapse brought on by too hard work, and he could not accept the view that cystocele was an inevitable antecedent of prolapse. He had on occasion been reluctantly compelled to curette cases of severe haemorrhage of puberty, and was very glad to find that Dr. Fothergill supported this method of treatment when all other means had failed.

Professor CHIPMAN (Montreal) remarked that Dr. Fothergill was always original, and had lately done a great deal to improve their classification of pelvic disease. He asked whether it was not advisable to draw a distinction between development and growth, the former referring solely to the proper assembling of the several parts of the genital tract before the twenty-third week of intrauterine life, and the latter indicating an increase in the size of these parts. It seemed to him scarcely wise to group together a bicornuate uterus, which was certainly an error of development, and a cervical hypertrophy, which was rather an error of growth. He was greatly interested in the cause of the menorrhagia of puberty. Such cases in Canada were not treated by the curette, except as a last resource. A change in altitude of 800 to 1,000 ft. to a high and dry locality was advised, and had in several instances in his own practice afforded a very satisfactory result.

Dr. HEDLEY (London) asked whether Dr. Fothergill thought that supravaginal elongation of the cervix was commonly congenital. He considered it nearly always due to prolapse.

Dr. FOTHERGILL, in reply, agreed with Dr. Chipman as to the distinction between development and growth. He would be quite willing to call Group I "developmental abnormalities," and Group II "irregularities of growth." His patients with menorrhagia were, as a rule, workers, and in order to treat them by elevation he would only be able to send them to the top floor of the mill. He had never seen any evidence offered of the statement that elongation of the supravaginal portion could be caused by dragging from below. He frequently saw it entirely apart from prolapse.

SECTIONS OF PATHOLOGY AND GYNAECOLOGY AND OBSTETRICS.

(COMBINED MEETING.)

Professor HENRY BRIGGS, M.B., F.R.C.S., President of the Section of Gynaecology and Obstetrics, in the Chair.

DISCUSSION ON THE ETIOLOGY OF ECLAMPSIA.

OPENING PAPERS.

I.—J. W. BALLANTYNE, M.D., F.R.C.P.E.,

Physician, Edinburgh Royal Maternity Hospital; Lecturer on Midwifery and Gynaecology, Surgeons' Hall, Edinburgh, etc.

THE etiology of eclampsia deals with the investigation of the cause or causes of this dangerous complication of the pregnant state and with the mode of action of the cause or causes. There are thus two closely related paths of inquiry. Proceeding along one of these, we search out the circumstances which generally precede the appearance of convulsions, and which may possibly be causally connected with them; going by the other track, we try to discover how, by what mechanism, and in what manner, the cause, or the alleged cause, acts upon the patient's body and leads to the convulsive phenomena. The former method is, as I take it, *etiology* in the ordinary sense of the word, whilst the latter is best named *pathogenesis*. The former the profession shares with the general public, which speculates, often with amazing assurance, on such matters; the latter is the possession of the scientific physician, who leaves the surface of things and strives to go deep down among the roots of origin of the causes themselves.

The layman is sure that convulsions in childbed are due to something going wrong with the kidneys; the obstetrician is not certain as to the part played by the kidneys, and laboriously riddles his ton of earth looking for the diamond of fact lying hid somewhere in it.

Etiology.

It is unnecessary to spend much time over etiology in the ordinary and popular sense of the word. The eclamptic patient is either pregnant or is passing through the early days of the puerperium; one is justified, therefore, in regarding pregnancy as the great cause of eclampsia. But it is not a normal pregnancy which so relates itself to convulsions. It is one in which the intricate relationship which has been rightly called the harmonious symbiosis of pregnancy—that marvellous mechanism by which mother and fetus act, react, and interact upon each other with mutual benefit—is in some way and in some degree thrown into disorder and dissonance, and mutual harm results in place of advantage. It is generally a first pregnancy which thus breaks down into disaster; the presence of two fetuses in the uterus apparently makes the supervention of convulsions more probable; that the pregnancy is an illegitimate one seems to have the same effect; certain parts of the world and certain cities have more cases than others; and particular atmospheric conditions, especially such as throw a strain upon the kidneys, favour its development. To this picture of the typical eclamptic patient as an unmarried primipara or twin-bearing multipara, nearing her full term in cold foggy or warm muggy weather, it is permissible to add a few touches, and to say that she is habitually constipated and generally neglectful of the laws of hygiene in the matter of diet, exercise, bathing, and sleep. There may be a history of kidney trouble before marriage, but I lay no great stress upon that; uraemia may result from pre-existing chronic nephritis, but this, in my opinion, is a different thing from eclampsia. Other causes have been alleged, including such unexpected things as round worms, emotional strain, and malaria; but they have not come within my experience, and of the so-called neurotic type of eclampsia I have met only one or two doubtful cases, for it seems to be very rare in Edinburgh, at least.

Pathogenesis.

When we pass from etiology to pathogenesis we go from matters about which there is some degree of agreement to theories most of which rest upon slender substrata of facts, and many of which have led to very divergent views. Here we plunge into a veritable labyrinth of speculations. We are searching for what R. L. Stevenson called "the reason o' the cause and the wherefore o' the why," and we find very little else than the spaewife's answer, "it's gey an' easy speirin'," and the natural desire to go on asking. It is easy to express an opinion on causation (etiology), but it is often of small importance; it is extraordinarily difficult to state with assurance any single fact of pathogenesis, but the single fact may be of the value of the four or five grains of radium in the ton of uranium residues.

A Toxaemia, with Causes Outside the Uterus.

We may start from the standpoint that eclampsia is essentially a toxaemia, although there may possibly be some who place a query even here; and for the purpose of treatment there is, I feel sure, much benefit in so regarding eclampsia. This conclusion, however, is only a starting point, not a terminus, in pathogenesis. What is the cause of the toxaemia? To this deeper question many answers have been given. Some investigators have looked outside of the mother's body for the cause; they have

imagined a microbe coming from without, with a special tendency to infect pregnant women, causing the epidemicity or semi-epidemicity occasionally noticed in connexion with eclampsia; and, when hindered by a lack of evidence, they have suggested that a comparatively harmless bacillus may, when pent up in the uterus between the decidual membranes and the wall, become more virulent or virulent in a new way. But if there is one thing which may be described as fairly certain, it is that eclampsia is not of microbic origin; we may keep the question open for a little longer, for we have not forgotten about the long-delayed discovery of the syphilis organism, but we hardly look for the announcement of the arrival of the *Bacillus eclampsiae*.

We look next for a cause of the toxaemia in the woman's body, but outside the uterus; and we are offered a large number of morbid agents to choose from. We turn hopefully to the kidney, for we note the diminution in the amount of urine excreted, the presence of albumen and occasionally of blood and tube casts, the fall in the urea, and perhaps the decrease in urinary toxicity with increase in the blood toxicity; and we say to ourselves the kidneys are proving inadequate to their task of excreting noxious products, they have been incapacitated by nephritis, and some poisonous thing, such as urea or carbamic acid or xanthin or creatin, is being retained in the body and is causing toxaemia.

Now, whatever may be the pathogenetic value of renal conditions in eclampsia, there cannot be the slightest difference of opinion in respect of the importance of albuminuria as a warning of impending convulsions; it may well be called the C.Q.D. among the danger signals of the symptomatology of morbid pregnancy, and the medical man who does not respond when he discovers this message for help is risking the lives of mother and infant. Next, we may look at the liver, having our attention specially drawn to it by reason of the pathological changes found in it in fatal cases, and see in it signs of inadequacy; or we may put the blame upon the intestines, being impressed by the extraordinary constipation which prevails in pre-eclamptic patients, and saying to ourselves that there must be great absorption of toxins taking place from the vast area of the intestinal mucosa; or, again, we are led to look at the thyroid gland, not unwillingly, for we share with the physician a great hopefulness that there is a sort of therapeutic El Dorado in the ductless glands, in those formerly unconsidered trifles of parenchyma which are now seen lording it over the metabolism of the body and saying, "We are both the cause and the cure of many maladies; blame us if you so please, but use us also." Now, latest of all, we have the suggestion that not thyroid but parathyroid inadequacy is the root cause of eclampsia, and since this theory has had the effect of slightly modifying my plan of treatment, I may refer to it a little more in detail.

It is known that parathyroidectomy in animals is followed by twitching of all the muscles of the body, then by tonic contractions and dyspnoea, and then by death; there is also a rapid excretion of lime salts constituting a sort of calcium diabetes; along with these marked changes there are also the signs of acidosis. The appearance or the evolution of these phenomena can be checked by the injection of lime salts into the peritoneum.

It has been found, further, that partial parathyroidectomy may be followed by no evil results unless pregnancy supervene, and that tetany of the nursing woman would seem to depend in some way upon the strain of lactation. It does not, of course, follow that tetany due to experimentally produced parathyroid inadequacy is the same thing as eclampsia in the pregnant woman, but some evidence has been collected in support of such a conclusion by Vassale and others. It has been stated that the parathyroid glands are more active during gestation; that the removal of them during gestation is followed by more severe symptoms than in the non-pregnant condition; that in some cases of eclampsia *post-mortem* examination has shown a diminution in the number of the parathyroids or signs of defective functioning in them; that some women suffering from maladies of the thyroid gland (whose effects could scarcely be separated from those due to parathyroid disease) have developed eclampsia or tetany in pregnancy, and that some of the good results which have occurred after the giving of thyroid extract in eclampsia have been due to the presence in it of parathyroid gland. It is an ingenious theory, although hardly securely enough grounded to enable us to accept it as more than a possible explanation of eclampsia, or as a probable solution of some of the morbid states associated with eclampsia; for the results of parathyroid anomalies or lesions are not the same thing as eclamptic seizures. It has been, however, the cause of my trying the effect of chloride of calcium, given intravenously, during eclampsia.

Another gland with an internal secretion which has been by some regarded as a cause of eclampsia is the suprarenal capsule; and the high blood pressure along with the cardiac hypertrophy of pregnancy give some support to this view. Others incline to incriminate the pituitary gland. Yet others look for a cause in a sort of mêlée or irregular conflict between all the glands with internal secretions. Here also we may refer to Sellheim's mammary theory of the origin of eclampsia, based upon the remarkable resemblance which exists both in symptomatology and in morbid anatomy between calving fever or parturient apoplexy in cows and convulsions in the parturient woman; this theory has brought along with it the amazing suggestion that amputation of the breasts should be practised to control eclamptic seizures; and the suggestion has actually been carried out.

Now, all these theories look to an organ outside the genital sphere as the seat of the cause of eclampsia, but into every one of them *pregnancy* enters. The foreground of each etiological picture, so to speak, is occupied by different characters, but the background is always the same. The kidney may be the failing organ, but it is the kidney as influenced by gestation which is understood; or blame may be attached to the liver, or the thyroid, or the parathyroid, or the adrenal, but it is always the liver, thyroid, parathyroid, or adrenal of pregnancy which is incriminated. This consideration or generalization leads us, therefore, naturally enough to the next group of theories—those which look inside the uterus, or, at least, inside the genital sphere, for the primary cause of the toxaemia which produces eclampsia, and which are content to regard the failure of the other organs (kidneys, liver, parathyroids, etc.) as the cause of a secondary toxaemia—important enough, and only too fatal sometimes, but yet secondary. We may, perhaps, think of the two groups of pathogenetic views as linked together by the theory of mammary causation, for the breasts are in more intimate sympathy with the utero-ovarian system than any other organs in the body.

Within the genital sphere a cause has been found in the ovary, or, rather, in the corpus luteum in the ovary; and some have begun to think of this mysterious, and possibly potent, structure as a sort of controlling centre under which the activities of the ductless glands, and possibly also of the placenta, are increased or diminished; it may be the place of origin of a hormone, and this hormone may stimulate the mammary glands; and there are other "may be's," but the corpus luteum still remains a force of unmeasured significance for good or ill.

A Toxaemia, with Causes Inside the Uterus.

When we turn to the contents of the uterus as possible factors in the production of eclampsia, we are at once embarrassed by the number and the intricacy of the suggestions. We wander somewhat hopelessly among the fetal, the placental, and the amniotic theories; among the notions of poisons generated in the fetus, in the placenta, or even in the liquor amnii; among the highly hypothetical ideas of exaggerated formation or penetration of plasmodium, of haemolysins, and of antibodies or syncytiolysins; among the views which seem to regard the fetus *in utero* as a prejudicial parasite against which the mother has to be elaborately protected by a complex system of blood and glandular reactions, and those which on the whole look upon the unborn infant as a healthy stimulus of the maternal tissues becoming dangerous only when the infant or the mother is already weak or unhealthy. In other words, the hypotheses as to the intrauterine or ovular causes of eclampsia raise anew and in acute form the whole problem of the intimate nature of pregnancy; there is a real lack at the present time of a correlation of our knowledge or partial knowledge of the biology of reproduction with the pathology of eclampsia. What, for instance, is the significance of the fact that eclampsia may

be associated with hydatidiform degeneration of the chorion and absence of the embryo? It must have a meaning; it seems at first sight to go against the fetal theory of origin, but on closer inspection it rather tends to concentrate our attention upon an intrauterine cause. What, again, is the reason why 20 per cent. of eclampsias are *post partum* in time? Surely the cause of these cases must be extra-uterine, for the uterus has been emptied before they develop; but there is the possibility of delayed action of pathogenetic factors to be considered before we can decide with any certainty.

A New Question: What is the Mechanism of the Normal?

It seems to me, therefore, that investigation must be given a new direction. We have been working for years at the question why it is that one pregnant woman in five hundred suffers from eclampsia; let us now ask ourselves persistently why the four hundred and ninety-nine pregnant women remain healthy, so far at least as convulsive attacks are concerned. Let us first make an exhaustive inquiry into the physiology of pregnancy and into the natural inter-relationship which exists between the fetus and the maternal organism. A good beginning has been made, and every month witnesses additions to our knowledge of the chemical interchanges of gestation. We know now with certainty that the pregnant state has an immediate, a profound, and a far-reaching effect upon every organ and tissue of the mother's body. The presence of the impregnated ovum *in utero* affects at once the growth of the most distant parts of the woman's body, for example, the nails, so that literally she may be said to tingle to her very finger-tips with the new life that is in her. Such epoch-making books as Professor Bar's work, *Leçons de pathologie obstétricale*, which deals, let it not be forgotten, with the physiology as well as with the pathology of pregnancy, have done much to throw light into a neglected corner of the science of life. I, for one, fully accept Professor Bar's great conclusion that when there is a healthy fetus in a healthy mother (*fetus sanus in matre sana*) there is developed an instance of harmonious symbiosis of the most wonderful kind, and that at the end of such a pregnancy the mother emerges no worse, but rather the better, for the strain which has been placed upon her organs and tissues. No one who has studied Bar's numerous analyses of the nitrogen, calcium, iron, phosphorus, sulphur, sugar, fat, and chloride interchanges between mother and unborn infant can any longer continue to regard the latter as a necessary danger to the former, or to look upon pregnancy as a state of prejudicial parasitism. At the same time, just because the relationship is so vital and so intricate, it is very delicately balanced; a little deviation from the normal in either the mother or her fetus may dangerously disturb the equilibrium and convert the natural harmonious symbiosis into the unhappy association of morbid antenatal and antepartum tendencies. In eclampsia there is, I think every one will admit, a break in the harmonious symbiosis of pregnancy; when we have discovered all the mechanism by which the normal state is ordinarily maintained we shall be infinitely better fitted to pick out in any given case the defective or disordered part which has caused the breakdown which has eventuated in convulsions.

Let us, therefore, welcome all pieces of work which tend to throw light upon any part of the physiology of pregnancy, and let us try to piece them into the great scheme of antenatal health and maternal safety in gestation. There is, for instance, Abderhalden's recent research upon the protective enzymes of the maternal blood in pregnancy; if foreign proteins, fats, and carbohydrates be introduced directly into the blood, enzymes appear adapted to digest these unusual components of the circulating fluid. Further, in pregnancy Abderhalden found indications of the presence of enzymes in the blood capable of digesting the protein derivatives of the placenta; these were not found in the blood of non-pregnant women. Obviously this is a very important matter, and must have a bearing upon the theories of eclampsia which look upon the invasion of the maternal blood by chorionic or placental tissue as a possible primary cause of the toxaemia which leads on to the convulsions. Every such piece of information is of possible etiological importance. The investigation of the antibodies of the blood in pregnancies may do much to clear up the pathogenesis of eclampsia, and may give us, if it has not already done so, a physiological blood test of pregnancy; the determination of the histological appearances of the parathyroids in the pregnant state, and their comparison with the changes found in eclamptic cases, may assist to establish or to disprove the parathyroid theory of the disease; and the calculation of the normal demands of the fetus for calcium, iron, sodium chloride, etc., when placed alongside of analyses of the maternal urine and blood at various dates in gestation, may at once give a hint as to the pathogenesis of eclampsia and a suggestion as to its treatment. Until we know as much about fetal physiology and the associated maternal reactions as we do about, say, digestion in the stomach and duodenum, we shall hardly be able to decide finally regarding the disturbance in the feto-maternal symbiosis which lies at the root of the origin of eclampsia.

The Argument from Treatment.

In the meantime our comparative ignorance of pathogenesis is no excuse for a suspension of attempts to reach a satisfactory line of treatment. Indeed, we can to a certain extent argue backwards from successful treatment to a probable pathogenesis. During the seven years in which I have been in charge of the Edinburgh Royal Maternity Hospital in the autumn quarter—a period altogether of twenty-one months—I have dealt with 43 cases of eclampsia; during the first three years I employed medical means of treatment, but not to the exclusion of forced delivery in some instances, and had a maternal mortality of 3 in 12 (25 per cent.); during the last four years I have left the uterus alone and have concentrated my energies on the medical treatment of the fits, and have had a maternal mortality of 3 in 31 (9.6 per cent.). The medical treatment was purely antitoxaemic, consisting of venesection, intravenous transfusion, stomach washing with sodium bicarbonate solution (1 drachm to the pint), the running into the stomach of 6 oz. of magnesium sulphate, the giving of a copious enema, and the use of the hot pack. Chloroform was only used to enable us to carry out the other measures and to control the fits before the medical means began to take effect. Morphine and thyroid were occasionally employed. I feel confident, therefore, that the great cause is a toxaemia, although I am not yet certain whether it is a primary or a secondary toxaemia which is immediately responsible fer producing the fits. I am inclined, further, to believe that the causal toxaemia is somewhat slowly produced, for the reason that I have found that when the above-named measures have been taken the fits do not return even when the pregnancy persists for a few days. I am therefore inclined to think that the shock accompanying rapid emptying of the uterus is a greater cause of danger than the possible continued influx of toxic substances due to the persistence of the gestation.

The only change I have recently made in the details of treatment has been the partial substitution of a solution of calcium chloride for the saline in the intravenous transfusion. I have been led to make this change because of a very satisfactory result following the use of this means in a serious case of chorea gravidarum. The patient was at the sixth month of pregnancy, and was suffering from chorea in an aggravated form. The washing out of the stomach, accompanied by the transfusion of a pint of solution of calcium chloride (30 grains to the pint) into a vein of the arm, was followed in a few hours by marked improvement, and the patient has since had no further treatment but calcium chloride by the mouth in 20-grain doses four times a day. It is now (three weeks later) difficult to detect any choreic movements at all. The pregnancy is continuing and the fetus is alive.* Possibly the calcium chloride has acted as a sedative to the nerve cells; at any rate, it seems justifiable to give the drug a trial in eclampsia.

CONCLUSIONS.

The chief factors in the etiology of eclampsia are pregnancy — especially, but not exclusively, a first pregnancy—renal and hepatic inadequacy, and possibly certain external influences and internal disturbances

*Now, three months later, there has been no return of the chorea, and the patient is near her delivery.

which throw a strain upon the kidneys and the nervous system.

The pathogenesis is most satisfactorily to be regarded as a toxaemia; probably there is a primary toxaemia due to some defect in the normal harmonious symbiosis of fetus and mother, and possibly the convulsions themselves may be caused by a secondary toxaemia, which is the result of, or has been caused by, the morbid changes which are often found in the kidneys, liver, parathyroids, adrenals, etc.

For the further elucidation and explanation of eclampsia reliance must be placed upon the investigation of the intricate feto-maternal relations which exist normally in pregnancy; when the physiology of gestation is known as thoroughly as that of digestion, it may be possible to reach some unassailable conclusions regarding the pathology of the pregnancy which is accompanied by convulsions.

In the meantime, the theory of a toxaemia is supported by the good result obtained the use of purely antitoxaemic measures; when these are successful the continuance of the pregnancy does not apparently bring any immediate dangers with it, possibly because the primary toxaemia takes some time to reproduce itself; the ideal treatment aims at the stopping of the fits and the continuing of the pregnancy, with a living mother and a living fetus.

II.—John H. Teacher, M.D.

It is generally agreed that while the pathological picture varies greatly in different cases of puerperal eclampsia, it is sufficiently precise and definite in its principal features. These are, speaking generally, degenerative changes of the nature of cloudy swelling and patchy necrosis, frequently with capillary thrombosis and haemorrhage, occurring in many organs, but most striking and characteristic in the liver.

During the past three years the *post-mortem* examinations at the Glasgow Maternity Hospital have been made by me or my assistant, Dr. A. M. Kennedy. We have not made them the subject of a systematic research, nor have we examined sections in all cases, because we soon came to the conclusion that we could in the main accept the conclusions set forth in Schmorl's monograph. We have seen a certain number of cases in which convulsions were associated with the puerperal state, but which could not be called cases of puerperal eclampsia, being clearly uraemic or due to some lesion of the nervous system. Excluding these, a larger group remains which we regard as puerperal eclampsia, in number 21 cases.

In all of these there were pathological changes visible to the naked eye. In six they were cloudy swelling in the kidneys and cloudy swelling and fatty change in the liver, and in one or two they were not at all striking. Twelve showed, in addition, the characteristic haemorrhages into the liver, and in six of these there was also cerebral haemorrhage. The remaining three showed symmetrical necrosis of the cortex of the kidneys, and were cases of eclampsia with suppression of urine, but without the usual uraemic phenomena.

In what we regard as the most characteristic cases the liver is covered with scattered red, irregular patches, due to haemorrhage immediately underneath the capsule. These are largest and most numerous on the right lobe. The substance of the liver is mottled with haemorrhagic areas, singly of small size but often confluent and very striking, sometimes bright red and at other times dull red in colour, showing that they were at least a day or two old. Anaemic infarction of the liver in addition to the haemorrhagic foci has been described but is rather rare. I have not seen it.

Microscopically the changes are found at the periphery of the lobules, that is, close to the portal tracts, and this distribution is characteristic of the disease. The earliest changes are degenerations of the liver cells, either shrinkage with defective nuclear staining, even to the extent of complete necrosis, or swelling and vacuolation, which resembles fatty infiltration, but is not of that nature. In the latter condition the cells become very greatly enlarged and foam-like, lose their nuclear staining, and are invaded by leucocytes. The capillaries between the cells may be dilated.

The next stage is the development of the areas of haemorrhagic necrosis. The capillaries are overdistended with red blood corpuscles closely packed together, small haemorrhages occur, and thrombosis takes place with formation of fibrin within the vessels, and it is also deposited outside them among the degenerated cells. The red corpuscles degenerate and become shadowy, their haemoglobin dissolving out and forming granules of brown pigment, the fibrin contracts into solid masses, and a structureless necrotic focus is the result.

Round these foci great dilatation of capillaries may develop with atrophy of the liver cells between them. This is probably a secondary result of the thrombosis of the first affected capillaries. When the thrombosis is extensive and affects the larger vessels in the portal tracts anaemic infarction of large masses of liver tissue may take place. The rest of the liver commonly shows cloudy swelling.

Similar changes have been observed in the suprarenals.

The changes in the kidneys are said to be even more constant than those in the liver. They consist of cloudy swelling and patchy necrosis of the epithelium of the convoluted tubules and ascending loops of Henle. The necrosis is regarded as the characteristic lesion. Haemorrhage in and around the necrotic areas and capillary thrombosis and small infarctions also occur. The glomeruli are swollen in some cases, but show neither proliferative changes nor cellular infiltration.

The symmetrical necrosis found in some cases associated with suppression of urine may be related to the last-mentioned phenomena. It is an anaemic necrosis or infarction affecting about two-thirds of the whole renal cortex in both kidneys, and it is caused by thrombosis in the interlobular arteries, the peculiar distribution of which is best explained on the theory of spasmodic contraction affecting these arteries and so favouring the formation of the localized thrombi. The condition may be compared with the white infarction of the liver above mentioned. Up to 1910 only six cases of this kind had been recorded, two of them by Dr. Robert Jardine and myself; but since then we have seen three more in Glasgow. The condition is probably less rare than has been supposed. An important point is that the fits generally ceased as the suppression of urine developed.

Schmorl also described capillary thrombosis and focal necrosis in the lungs, intestines, heart, and brain, frequently accompanied by haemorrhage. Larger cerebral haemorrhages are not uncommon; and their principal characteristic appears to be that they occur in other than the usual situations of cerebral haemorrhage. Similar lesions in the organs of the fetus have been described.

As to the placenta little can be said. Schmorl has long ago shown that the pulmonary embolism by masses of syncytium, which he was the first to describe, and which he suggested might be the cause of puerperal eclampsia, is a common feature of pregnancy. Degenerative changes are found in the organ itself, but they are not recognized as peculiar or really different from those which appear so commonly without definite disturbance of pregnancy.

As to the significance of the pathological changes. Every one agrees that there is no relation between their extent and the severity of the fits. There may be little change in cases with enormous numbers of fits, and, on the other hand, profound changes in the liver have been found in patients dying in the puerperium without fits—eclampsia without convulsions.

The general opinion appears to be that the morbid changes are, like the convulsions and other features of the disease, an expression of a general intoxication presenting resemblances on the one hand to uraemia and on the other to the intoxication of septic disease. Doubtless they produce grave disturbance and are unfavourable to recovery, but they are not the cause of eclampsia.

The peculiar character of the distinctive lesions may be referred to the high clotting power of the blood which is present in pregnancy, coupled with the injury to the walls of the small blood vessels by the unknown toxin.

III.—Sir William Smyly, M.D.,

Gynaecologist to the Adelaide Hospital, Dublin.

THE APPLICATIONS OF RECENT RESEARCH IN THE TREATMENT OF ECLAMPSIA.

The term "recent research" suggests the laboratory work of chemists and biologists, and without doubt their work has done much to assist practitioners in forming some idea as

to the nature of the conditions which are present in eclampsia. But they have not up to the present time discovered the cause of these conditions, and therefore, as regards treatment, there is no clear light or leading to be got from them. Instead, they have supplied us with an abundance of theories, more or less probable, which have very materially affected our practice. Practice is the test of theory, and is, in the absence of certain knowledge, purely experimental: it should, therefore, be included amongst the methods of research.

The treatment of eclampsia at the present time appears to me to be influenced by three considerations: (1) The particular theory adopted by the practitioner; (2) the urgency of the symptoms; and (3) the results of previous experience.

I propose, therefore, to consider the subject in this order.

Taking, then, the influence of theory upon practice, I begin with the idea that this disease is due to a poisoning of the maternal system. The evidence in favour of this theory is so strong as almost to amount to certainty, and it is generally assumed by therapeutists to be a fact, and therefore their efforts are generally directed towards:

1. Preventing the formation of toxins.
2. Neutralizing or destroying those already formed:
3. Eliminating them from the system.

1. In order to prevent the formation of toxins we should have some idea as to where they originate. They must do so either in the mother or the ovum, but in which or in what part of either we do not know. Those who believe that the ovum is the source of the poison consider that the only reasonable course of treatment is to get rid of it as quickly as possible, and with this object the cervix is dilated with the fingers, rubber bags, or metal dilators; and the child delivered by version and extraction, the forceps, or the cranioclast. The brilliant results which were obtained by Dührssen in cases in which he extracted the fetus after deep cervical incisions or vaginal Caesarean section strengthened and appeared to confirm the ovular theory; and he went so far as to recommend that in every case of eclampsia at the end of pregnancy, or *sub partu*, delivery should be effected as rapidly as possible, and, if possible, after the first fit. Out of 80 cases treated in this way he lost only 15 per cent.

His results had such influence upon practice that by many it was held that to fail to deliver an eclamptic patient as rapidly as possible was little short of malpraxis. Albeck thus analyses the results in 136 cases treated actively to show the importance of early delivery:

Where the Mother was Delivered.	Maternal Mortality.	Fetal Mortality.
	Per cent.	Per cent.
After 1 fit	14.3	16.0
,, 2 fits	10.0	37.0
,, 3 to 10 fits	16.0	44.0
Later...	44.8	69.9

Total, 136 cases. 19.8 per cent.

Dr. J. A. Mitchell and Dr. Jennie Drennan attributed the eclamptic condition to the abstraction of calcium salts from the mother by the fetus, and recommended calcium lactate as a remedy. The fact, however, that eclampsia occurs *post partum* and in cases of molar pregnancy was against any theory which involved the fetus as the only source of the poison, and concentrated the attention of those who hold the ovular theory upon the placenta. It was quite natural for those who regarded this structure as the source of the disease to assume that when convulsions commenced or continued after the expulsion of the placenta, part of it must have been left behind; and consequently the uterus was curetted in several cases with gratifying results. It is important, however, to note that the microscopic examination of the particles removed failed to show any fetal structures.

Those who hold that the poison originates in the maternal organism are just as much at variance in practice as in theory. If, for example, the poison is due to some ferment derived from the ovum, the only rational treatment is to get rid of the latter as quickly as possible, whereas if it be due to some hitherto undiscovered micro-organism, the condition will tend to cure itself if the patient can only be kept alive until this is accomplished.

Those who, like Kaiser and Strobach, would locate the mischief in the ductless glands, have given thyroid and parathyroid extracts. The mammary theory was started by Bolle from the similarity between the symptoms in eclampsia and in a disease which occurred in cows, and in which the mortality had been greatly reduced by injecting a solution of potassium iodide into the udder. He tried the same treatment in seventeen cases of eclampsia in women, and his example was followed by Sellheim, who, as we know, went further and excised the breasts. Dr. Emilio Santo recommended injection of sterilized air, and others massage and Bier's suction treatment.

Dr. Hastings Tweedy, at the Birmingham meeting last summer, stated that the intestinal tract was the source of the poison, and very strongly insisted upon the necessity of thoroughly washing out the stomach and rectum, and of withholding every form of food, even milk in the smallest quantities. We have long been familiar with the advantages attending a restricted diet in cases of threatened eclampsia, but milk has hitherto been regarded as almost a specific in its prevention. Tarnier, who introduced this treatment in 1875, stated positively that eclampsia would not occur where a patient had been restricted to milk for a week. And Pinard has lately said that during thirty-five years' experience he had never seen a patient get eclampsia who had been on a milk diet for eight days. The fact, however, which I think is generally admitted, that these patients do better on milk than on any other diet, does not prove that they might not do better still on nothing but water. But it does prove that the intestinal contents are an important factor in causing the disease. Dienst also appears to attach importance to the intestines, for he regards vegetable acids as important prophylactics. He says that in Leipzig vegetable salts are administered with the object of neutralizing organic acids, and especially lactic acid, in the maternal system. But he believes that the good results obtained by this treatment are attributable not to that alone, but, as Noff has proved by experiments upon animals, that by filling their intestines with vegetable acids, the hepatic lymph became uncoagulable, so in the human subject these acids, by increasing the secretion of the intestinal glands, stimulate the liver to increased production of antithrombins. He therefore advocates giving such drinks as lemonade not only in eclampsia but in all cases of thrombosis.

2. As regards the neutralization or destruction of the poison in the maternal system. Assuming that in health this process is being constantly carried out by certain antibodies, and that toxaemia is the result of insufficiency of such antibodies, Mayer has infused serum obtained from healthy pregnant women; and, with the same object, Freund and others have employed horse serum, which has the advantage of being more easily obtained, whilst Leith Murray has suggested antivenin.

3. The elimination of the poison is assisted by increasing the action of the skin, the bowels, the kidneys, and by abstraction of blood. Opinions are divided as to the value of diaphoresis; in my own opinion, it is more likely to do harm than good by abstracting water from the blood, which, as Zweifel has lately proved, is thicker than normal in eclamptics. It is better to introduce plenty of fluid into the system, through the stomach or rectum, hypodermically or directly into a vein. Normal saline solution is usually employed, but though many of the best authorities declare that they have never seen anything but good result from it, yet the danger of chloruraemia in eclampsia is a fact which must be weighed against the use of chlorides, and it is probably better to replace the sodium chloride with either the bicarbonate or sugar. The fluid acts beneficially in many ways, not the least of which is by promoting the action of the kidneys. In the same direction decapsulation of these organs, first introduced by Edebohls, appears to have been of value, but should only be resorted to after delivery in cases of anuria where every other means have failed.

PHLEBOTOMY.

Fifty years ago bleeding was regarded as the sheet anchor in the treatment of puerperal convulsions, but, though never completely abandoned, was until quite

recently but seldom employed. Now, however, a reaction seems to have set in in its favour, and from France, Germany, and America a loud chorus is raised in its praise. Commandeur declares that it is the most important remedy at our disposal, and Lichtenstein endeavours to prove from statistics that all the benefits derived from operations are attributable to the attendant haemorrhage. The important thing, he says, is not emptying the uterus, but loss of blood. In his statistical tables he shows that after operative delivery convulsions ceased in one-third of the cases, and that those were the cases which had lost most blood. Bleeding benefits the patient by removing a certain amount of poison, by lowering blood pressure, and by assisting the secretion of urine.

The minute thromboses found in all cases of eclampsia are by some regarded as the cause of the disease. Dienst is of this opinion, and to prevent their formation recommended a diet free from sodium chloride and intravenous infusion of hirudin. The latter substance has also been employed by Engelman, Stade, and others, with success. It does not, however, dissolve thrombi which have already formed, and in one case at least it caused dyspnoea and serious symptoms of collapse. It should, therefore, only be employed early, before thrombosis is extensive or heart failure has begun. Other disadvantages are that it requires to be freshly prepared, and is therefore difficult to obtain in an emergency; and that it is very expensive.

Zangemeister believes that the convulsions are caused by pressure on the brain due to oedema, and to relieve that pressure he trephined the skull and laid open the dura mater. In three cases thus treated the convulsions ceased, but one died on the sixth day without any further fits; and he believed that the fatal result was due to his having closed the dura mater after the operation in that case.

Lumbar puncture has also been performed and successes recorded, but it has not met with much support.

The last theory to which I shall allude in connexion with treatment is that the disease is essentially a spasm of the minute blood vessels, and that, the other anatomical changes being secondary to this, the chief aim should be to prevent or relieve spasm by narcotics. The two narcotics most generally used are morphine and chloral, the latter, according to Commandeur, being regarded in France as almost a specific. Chloroform is for many reasons objectionable, and has been generally abandoned excepting for operations, or in very small quantities where for any reason the patient has to be disturbed.

Symptomatic Treatment.

Symptomatic treatment is of the greatest importance, for many of these cases die from accidents which could easily be prevented by a watchful attendant. The patient should be guarded from injury—especially from biting her tongue or inhaling the saliva and mucus which accumulates in her pharynx. A suitable gag should be held in her mouth, and her throat mopped out with gauze or cotton-wool; or, what is far better, keeping her on her side, and occasionally depressing her head over the side of the bed with the face towards the floor, as recommended by Tweedy; the mucus will then pour out of her mouth. It is further advisable to give oxygen after each fit.

High arterial tension is a common and very dangerous condition. It can be reduced by phlebotomy or veratrum viride. The latter drug, which was introduced in the United States by Baker half a century ago, has latterly been extensively employed in that country, and with considerable success. Gustave Zincke, of Cincinnati, recommends Norwood's tincture in 20-min. doses, given hypodermically every hour until the pulse is reduced to sixty beats per minute. When the pulse begins to rise he gives 10–15 min. every hour until it again falls to sixty.

Having considered separately most of the methods in vogue for the treatment of eclampsia, let us now try to treat the subject synthetically. It is quite evident that it would be impossible to employ them all, and it is equally evident that each practitioner must select those which appear to him to be the best, and combine them in a system which he shall be immediately prepared to carry out when the emergency arises.

In determining the value of any system of treatment where, as in eclampsia, we have no exact scientific knowledge to guide us, we are forced to fall back upon clinical experience. But eclampsia is a disease of such varying severity that we are liable to meet with long runs, sometimes of twenty or thirty cases, without a death; and then a series of great severity with many fatalities. So that to form anything like a definite opinion upon any particular line of treatment some hundreds of cases are required; and as so many cases are not likely to be met with by any one individual we are forced to fall back upon statistics. In making use of statistics two things are necessary: they must include a large number of cases, and the cases must be similar, so as to be comparable.

In a paper read recently in New York by Dr. Ralph Waldo Lobenstein he quoted the following, amongst others, to prove that the results of operative treatment are better than those obtained by medical.

Conservative Methods.

C. Zinke, 15.38 per cent. mortality; Stroganoff, less than 7 per cent.; Ahlfeld, 12 per cent.; Pfannenstiel, 35 cases without a death; Tweedy, 8.22 per cent.

Radical Methods.

United States generally, 10.25 per cent.; Dührssen, 112 cases and 15 deaths; Fromme, 34 cases without a death; Winter, 22 without a death; Veit, 33 with 1 death; Liepman, 2.8 per cent. by early, 25 per cent. by late, delivery.

A little investigation is sufficient to show that these figures are not comparable. For example, C. Zinke is tabulated as having a mortality of 15.8 per cent. amongst cases treated conservatively. But on referring to his paper I find that out of nine deaths one was due to haemorrhage after violent efforts at delivery; another occurred after *accouchement forcé*, and a third from septic infection. Of the cases allowed to deliver themselves the mortality was 8.3 per cent. Liepman's 2.8 per cent. cannot be compared with Stroganoff's 7 per cent., which was the total mortality out of 900 cases; whereas Liepman's 2.8 per cent. was obtained by a process of classification, and the total mortality was about 19 per cent.

In systematizing our methods of treatment, the first point of importance is to determine whether we shall or shall not include immediate delivery. The radical school have of late materially improved their results by the introduction of vaginal Caesarean section, but such a method is only possible in hospitals, and even there it has failed to give any better results than more conservative measures. And I believe that Commandeur is right when he says that the statistics which should have proved the value of active treatment have failed to do so.

The general practitioner who feels it incumbent upon him to deliver his patient as rapidly as possible generally resorts to *accouchement forcé*—that is, by dilating the cervix, turning, and extracting the child. That, in my opinion, is the worst method of all. The cervix does not stretch like a piece of elastic; on the contrary, it is apt to tear like wet blotting paper, and the proportion of cases in which serious lacerations occur is estimated by Bar as 30 per cent.

Accouchement forcé has increased the mortality in every condition in which it has been adopted. Forty years ago it was the recognized practice in placenta praevia, and the mortality was enormous, much greater than in accidental haemorrhage, in which the practice was to rupture the membranes. Twenty years later the practice was reversed; in placenta praevia the child was turned but not extracted, whilst in accidental haemorrhage it was; and at that time the latter was looked upon as a far more serious condition than the former. I believe that in eclampsia also *accouchement forcé* kills more than it cures.

The best results hitherto obtained have been by the methods adopted and systematized by Dr. Tweedy and Professor Stroganoff—the former with a total mortality of 8 per cent., and the latter with a little over 7 per cent. When the large numbers of cases and the uniformity of the results are considered it cannot be doubted that no other statistics hitherto published can show results comparable to those attained by Stroganoff's method. In a paper published last month in the *Centralblatt für Gynaekologie*, he reports 561 cases—600 treated in Russia with a maternal mortality of 8 per cent. and a fetal mortality of 21 per cent.; and 61 cases treated in Germany with a maternal mortality of 6.5 per cent. and a fetal mortality of 18 per cent.; and he reiterates his conviction that in uncomplicated cases in which his treatment is commenced

early the mortality should not exceed 2 per cent. When in Dublin the other day he informed me that he has now reports of over 900 cases, with less that 8 per cent. mortality.

In conclusion I may add the main features of this method as summarized by Stroganoff himself:

1. Avoidance as far as possible of all external irritation. Therefore a quiet darkened room to which no one is admitted but the doctor and nurse, who must be as silent and noiseless as possible. External and internal examinations restricted to what is absolutely necessary. These and all other manipulations, such as catheterization and hypodermic injections, are carried out under slight chloroform anaesthesia; generally 20 to 30 drops suffice.

2. The control of fits by morphine and chloral hydrate; given as follows in an average case:

1st hour,	0015	gram	(¼ grain),	morph. mur. hypodermically.
2nd ,,	2	,,	30 ,,	chloral hydrate per os or rectum.
3rd ,,	0015	,,	¼ ,,	morph. hypodermically.
7th ,,	2	,,	30 ,,	chloral hydrate.
13th ,,	1.5	,,	25 ,,	,, ,,
21st ,,	1.5	,,	25 ,,	,, ,,

3. Labour is assisted but not forced.

4. Watching and stimulating the vital processes.

(1) *Respiration.*—Posture, careful cleansing of throat and nose. Pure warm air. Oxygen after fits.

(2) *Heart.*—Milk and saline infusion per os and rectum. If pulse is weak, digitalis and stimulants.

(3) *Kidneys and skin*, warmth and saline infusions.

DISCUSSION.

Professor WALKER HALL (Bristol) congratulated the openers of the discussion upon the broadness of their views, and joined with Dr. Ballantyne in urging that more attention should be directed to the physiology of pregnancy. In fact, he went further and suggested that it was the earlier months of pregnancy which called for closer investigation. The multiplicity of views and treatments pointed to the probability that if it were possible to forget what had been published and commence once more with a study of the bio-chemistry of the condition, there would be good hope for more direct work. He referred in detail to Emil Fischer's work upon proteins and Abderhalden's investigations upon protective ferments, and related some work carried out in his own laboratories upon the action of blood and tissue fluids upon polypeptides. The action of blood of pregnant and non-pregnant and post-abortional blood upon polypeptides prepared from placental proteins showed essential differences, while the milk during lactation corresponded with the blood of pregnant women. It was necessary to realize that, after all, the condition associated with eclampsia was cellular in character; it was important to recognize that cellular action was carried on in a fatty as well as a watery medium. The current measures of treatment seemed heroic with regard to single cells, and appeared to cause chiefly such intracellular disturbances as would result in altered metabolism of nucleins and cytoplasm and probably reduce the cell to insensibility. While the means at present offered the most promising results, it appeared probable that the advances in chemistry and chemical pathology during the next decades would furnish the possibility of a more specific line of treating the cellular deficiencies.

Dr. LEITH MURRAY (Liverpool) was surprised that no mention had been made of anaphylaxis in pregnancy. Very much work had been done in recent years on this aspect of the subject and on the immunology of normal pregnancy in general. Mosbacher and several others had produced undoubted evidence that there was homologous sensitization in pregnancy by placental elements. He (the speaker) did not consider that toxic symptoms following were anaphylactic. Cases of phthisis were sensitized to tuberculin, yet anaphylaxis was not the usual termination. The clinical evidence, too, was all against such a theory; a fall of temperature, a lowered blood pressure, and acute asthma were not found in eclampsia, while the *post-mortem* lesions in the liver and lungs were entirely different. Discussing a direct intoxication, he considered that much water had flowed under the bridge since Schmorl wrote, and that it was now possible to define the eclamptic toxin as one consisting of five elements — thrombotic, haemolytic, haemagglutinative, endotheliolytic, and neurotoxic. The polymorphism of lesion was produced by interaction of these—for example, a high haemagglutinating value leading to much focal necrosis in the periphery of the lobules. It was important to appreciate (1) that death might result with an *apparently* slight lesion in the liver—that is, necrosis prevented by a low agglutinative and a high lytic value, precisely as occurred in the case of many immune serums; (2) that death might result with an *undoubtedly* slight lesion in the liver—that is, an early lesion accompanied by fatal cerebral haemorrhage. He did not agree with Dr. Teacher that the lesion of eclampsia was absolutely unique, as in reptilion venom poisoning as a whole the same lesions were produced by the same five elements. He considered, then, that a bio-chemical search for a body of this nature was advisable, especially as haemolytic tests gave ample confirmation of this similarity.

Dr. J. S. C. DOUGLAS (Birmingham) said: The condition of eclampsia may be due to the failure of the pregnant woman to produce antibodies to toxins which are always formed during pregnancy, normal or otherwise, rather than to the formation of some abnormal toxin. On this supposition the advantage of blood-letting in eclampsia may be explained not so much by the removal of toxins from the blood stream, but rather by the increased volume of antibodies brought into the circulation by this process, since it has been shown by v. Schroeder that haemorrhage in actively immunized animals was followed by an actual increase in the quantity of antibody present in the circulating blood.

Professor HAULTAIN (Edinburgh) said eclampsia was a much debated subject. Upon two points only were they all agreed—namely, that it was due to pregnancy and to a poison. Its actual cause might be due either to defective excretory function of the placenta or to deficiency of general metabolism. Probably the latter, as proved by the excellent effect of milk diet, which could have no action on placental function. The action of the poison was probably, in the first instance, to increase arterial pressure; secondly, to irritate the kidney, causing albuminuria and acetonuria, passing on to defective secretion and suppression; and, thirdly, direct irritative action on the cerebral cells. The treatment he adopted was based on physiological principles, there being no direct specific antidote to the poison—first, to try and remove poison; secondly, to reduce blood pressure; and thirdly, to counteract nervous symptoms. If these failed, to remove the cause or terminate the pregnancy. Removal of the poison by purgation, washing out of the stomach, diaphoresis, and bleeding. The latter also acts by reducing blood pressure, and for that reason he did not transfuse saline solutions to raise it again. He had given up the use of chloroform, as it might increase the poison, acetonuria being met with in chronic chloroform poisoning. Morphine he had also ceased to use. As regards emptying the uterus, he certainly recommended it in cases where in spite of all means of treatment the fits continued, as it was removing the primary cause of the disease. Where labour had commenced this was simple, but before labour had commenced it was dangerous, but appeared to him the lesser of two evils. He had done three cases of Caesarean section for this condition with two favourable results. Eclampsia cases varied enormously in severity. In Scotland it was more frequent and virulent than in Ireland, and one could not compare their methods or results. In Scotland it was becoming a distinct scourge, and he thought it would be most desirable if some means could be devised whereby the pregnant poor might learn the necessity of putting themselves under care in all instances, so that their urine might be treated, and thus by the prophylactic treatment of milk diet ninety-nine out of every hundred cases of eclampsia would be prevented.

Sir JOHN BYERS said that the discusssion, so ably introduced, showed that eclampsia was still a disease of theories, and that treatment, based upon the working hypothesis of the condition being a toxaemia, was ahead of their exact scientific knowledge of the complication. He hoped for further knowledge from

the clinical physiological chemist. He had found that neither weather, nor fog, etc., had anything to do with its incidence. As to treatment, the great thing was attention to details—to treat the convulsions with morphine, to purge thoroughly, to starve the patient, and not to bring on labour, but to facilitate its progress when it began and when the conditions were suitable. He emphasized the great importance of the prevention of the complication.

Dr. Griffith (London) asked Professor Walker Hall if his opinion was that the so-called toxins were merely waste products of disordered metabolism, and not causative of the diseases attributed to them. No speaker had attempted to limit or define the disease under discussion; the convulsions which characterized eclampsia and were clinically its most alarming feature were but a single phenomenon present in a small proportion only of the group of cases in which the essential pathological changes were present in various forms or degrees. Those cases in which these changes were comparatively slight recovered, if allowed to, under the simple treatment of rest and warmth, thorough evacuation of the bowels, and milk diet. The very severe cases, with extensive destruction of liver, kidney, and cerebral tissue, with haemorrhages, could not recover under any treatment, and they could not expect even to save the life of the infant. The intermediate cases were those in which they were bound to use such measures which accurate observation and experience proved to be beneficial. He entirely agreed with Sir William Smyly that these measures should be as gentle and, as far as possible, in themselves free from risk, and he would suggest a rule applicable equally to cases of placenta praevia and accidental haemorrhage, Do not kill your patient by the means you adopt in your endeavours to save her life. While opposed to the risky practice of forced delivery, he would accelerate delivery in suitable cases, feeling confident that the majority of women were safer after delivery than before. In the cases where the symptoms developed in women known to be sufferers from chronic nephritis, he held strongly that no time should be lost, but the uterus should be emptied at once, without any regard for the life of the fetus. The further progress of knowledge of this disease must, as in so many others, depend on the investigations of the biological chemist, for it appeared to be certain that it was not the result of microbial infection.

Professor Chipman (Montreal) said he was interested in the proceedings, especially so as eclampsia was the subject of discussion at the last meeting of the American Gynaecological Society, held in Baltimore in May. In this way an opportunity was being afforded them to compare the findings of the British and American schools upon this interesting subject. He said that the main fact that emerged from those two meetings was a realization of the little that they actually knew. He quite agreed with Dr. Ballantyne and Professor Walker Hall in the plea that they made, that they must approach the subject from the beginning. The lines of approach were along those of physiology and physiological chemistry. Many workers in America were attempting such research—among these was Williams of Baltimore—and their results were promised next year. In Canada, in the matter of the incidence of eclampsia, they must occupy a midway position between Belfast and Edinburgh. Eclampsia was common in Canada, occurring most frequently in the autumn and winter months, and the number varied greatly in different years. It was of advantage, he thought, to group together in one's mind the various toxaemias of pregnancy—for example, vomiting, chorea, and eclampsia. These were probably all toxaemic, the result of one or more toxins accumulating in greater or less degree within the economy. Accordingly the patient should be watched, not for three months of her pregnancy, but for nine months. In Montreal they were accustomed to roughly divide the cases of eclampsia into two great groups: (1) Those cases in which the kidney was most affected, and (2) those in which the liver suffered most. These two groups could frequently be distinguished clinically. The latter group was, fortunately, less common, the prodromota were less pronounced, and the prognosis much more grave. The etiology of the two groups was probably the same. the difference between them being in all likelihood due to a larger dose of the toxin, or to a lower resistance or idiosyncrasy of the individual. As regards treatment, they adopted a middle course. In group (1), the milder type of cases, wise medicinal treatment usually sufficed. In group (2) the case was often desperate from the first; destructive changes were rapidly occurring in the liver and kidney, and it was here often wise to deliver the mother as rapidly as possible. In all cases the blood pressure was carefully measured at frequent intervals, and the readings made an index of the progress of the case. He thanked the profession of Great Britain for the honour conferred upon Canada and on himself in being made a vice-president of the Obstetric Section.

Dr. Amand Routh (London) hoped this discussion would stimulate chemical pathologists to work harder at the toxaemias of pregnancy. It must be remembered that there were other toxaemias of pregnancy besides that of eclampsia; hyperemesis gravidarum, chorea, albuminuria, diminished urea excretion, and insanity, were all due to autotoxaemia. Probably before the exact constituents of the toxins could be ascertained—and clearly they were different in each of these diseases—pathological chemists must first work out the normal difference in the blood in a pregnant as compared with the blood of a non-pregnant woman. They knew nothing of the chemical constituents the accumulation of which in the blood led to the onset of labour, and of lactation; they did not even know much of the cause of the cyclical onset of menstruation, though Blair Bell had shown that it was to some extent due to an accumulation of calcium salts in the blood. If, therefore, the physiology of pregnancy were more clearly understood, especially as regards the bio-chemical changes in the blood, they would then be on the way to discover the causes of the toxins. As regards treatment, he would only deal with one point, that of obstetrical interference, and he strongly deprecated any variety of *accouchement forcé*, but if the woman were in labour and the cervix dilating, he usually punctured the membranes, thus reducing intra-abdominal and, indirectly, arterial pressure, and when the head was on the perineum the forceps should empty the uterus under ether anaesthesia. He hoped for great results from the continued co-operation of clinicians, pathologists, and chemists, and this discussion was sure to further such a combination.

Professor J. M. Munro Kerr (Glasgow) said: As regards the etiology of the disease, the probability is that the start of the toxaemia is the destructive effect of the trophoblast on the uterine mucosa. Generally the maternal organism resists and overcomes, naturally or by medical means, the poison; sometimes it does not. If the latter occurs, then the pathological changes and clinical features with which we are familiar develop. As regards the treatment of eclampsia, I am a supporter of the British and French school, which relies upon medical rather than active surgical treatment. I therefore rely upon washing out of stomach and bowel, venesection, moderate saline infusion, and morphine. But I make this reservation: Every one is familiar with this fact, that every now and again the type of this disease is very virulent. Now, when one gets among such cases, I believe that active surgical treatment should be adopted, and adopted early. If the cervix is taken up, then incising the cervix and forcible extraction with forceps may be employed, but if the cervix is not taken up then I advocate abdominal Caesarean section. Vaginal Caesarean section is not an easy operation, and cannot be performed in a private house. Abdominal Caesarean section, on the other hand, is a simple operation. I feel convinced that the poor results obtained by Caesarean section could be improved by earlier operation.

Mrs. Scharlieb (London), narrating her experiences, said forty years ago (1873) in Madras they had a very heavy percentage of cases of eclampsia among their patients in the Government Lying-in Hospital. In contrast with Edinburgh, they had a hot, moist climate. In contrast with the inhabitants of Great Britain and Ireland, they had a rice-eating, total-abstaining population who ate no flesh food and who took no alcohol. In one particular only did the incidence of eclampsia in Madras agree with that at home. The patients were nearly always young primiparae, and often young primiparae pregnant with

twins or who had hydramnios. The puerperal convulsions in Madras were always associated with albuminuria and with pyrexia, frequently with jaundice and haemorrhages. *Post mortem* they found lesions of the kidneys and the liver. They had no sphygmographs or other instruments of precision, and they knew little about toxins. They acted on the belief that there was some poison in the woman's system, and that she resembled the strychnized frog in which convulsions were likely to be evoked by very slight causes. Consequently the treatment was eliminative and sedative. Seeing that any disturbance seemed to provoke attacks, they put the woman under chloroform, drew off the urine, and placed calomel gr. x and croton oil miij on her tongue. After this they gave chloral and potassium bromide, gr. xx each, by the nasal tube, and sometimes doses of veratrum viridis. As a rule they observed the maxim, "Take care of the fits and the labour will take care of itself." To this rule there was one exception. They ruptured the membranes, and of course, when necessary, gave help to the labour; but as a rule the uterine contractions were brisk and the labour progressed naturally. It was evident that this experience and treatment were clinical, not pathological, but it seemed as if they had progressed little during the last forty years. They must hope that collaboration of clinicians and pathologists and bio-chemists would be more fruitful before the end of another period of forty years.

Major R. HEARD, I.M.S., referring to remarks by Mrs. Scharleib, mentioned the prevalence of eclampsia in the hot, dry climate of Northern India, not only in the plains but also at higher altitudes. A point worth mentioning, in view of the remarks by some of the speakers regarding the value of milk in eclampsia, was the very large amount of milk in the dietary of the vegetarian races amongst which Mrs. Scharleib found the condition so frequent.

Dr. BALLANTYNE, in reply, was glad to have Dr. Walker Hall's support in what was really intended to be the central conclusion of his article, to wit, that the study of the physiology of pregnancy must be undertaken as the basis of further advances in their knowledge of eclampsia. Some of the plans of treatment might be of the nature of shock, but big diseases needed strong cures. Dr. Ballantyne thought that with regard to statistics no man had the right to speak of percentages unless he had had at least a hundred; etymologically that seemed to be a fair statement; but a man had every right to test the results of a change in his treatment, and to compare one set with another. They ought to elaborate the details of any kind of treatment, to feel free to alter small details from time to time, and to keep really an open mind on these matters.

Dr. TEACHER (Glasgow), in reply, was very interested to hear from Dr. Leith Murray that the liver in rattlesnake poisoning was so like that of eclampsia. The similarity of this intense toxic condition was interesting. Nevertheless, he was inclined to regard the comparison of the two conditions as a false trail. The morbid anatomy of snake venom poisoning was as variable as that of eclampsia. Dr. Teacher also referred to the placental theory of eclampsia, which he had omitted in giving his demonstration.

SECTION OF LARYNGOLOGY AND RHINOLOGY.

JOHN MIDDLEMASS HUNT, M.B., President.

DISCUSSION ON THE TREATMENT OF CHRONIC SUPPURATIVE ETHMOIDITIS.

OPENING PAPERS.

I.—Professor L. HAJEK,
Vienna.

THE methods of treatment of chronic suppurative ethmoiditis are, or ought to be, different, according to the pathological and anatomical conditions prevailing in the individual case. For the purpose of classification, we may put down the following groups of chronic suppurative ethmoiditis:

I. Chronic suppurative inflammation, with marked hyperplasia of the tissue.

II. Chronic suppurative inflammation of the ethmoid, with hyperplasia only in restricted areas, but with extensive atrophy (ozaena—Uffenorde,[1] Hajek[2]).

III. Secondary suppurative processes of the ethmoid consequent upon constitutional disease (syphilis, tuberculosis, and malign neoplasms).

I.

The cases belonging to Group I show either (*a*) that the hyperplasia predominates in the clinical picture, whilst the suppuration is circumscribed, often even latent and restricted to a few cells; or (*b*) that the suppuration predominates in the clinical picture (*a*). These cases are represented by the numerous well-known instances of chronic polypus formation of the ethmoid; the treatment is therefore coincident with the problem of treatment of chronic polyposis.

As regards this problem of treatment of polypi, which is, in fact, the most important part of the treatment of chronic disease of the ethmoid, I cannot undertake the task of enumerating all the various methods recommended for this purpose. I can only mention the leading ideas, which, in their turn, are intimately connected with the views pertaining to the formation of nasal polypi in general. There are undoubtedly cases of beginning and circumscribed polypus-formation, which do not return after removal even without resection of the osseous basis; this is the best proof that polypi originate from the superficial inflamed mucous membrane. We see very often after endonasal operations, that the mucous membrane produces "acute" polypi. These disappear after the irritation has subsided. Polypi forming around a foreign body do not return, as a rule, if they are removed together with the foreign body. All these facts are instances of superficial inflammation of the mucous membrane of the ethmoid region.

In the majority of cases with nasal polypi we see recurrences after a while, and the therapeutic methods depend upon the causes in the reappearance of polypi.

According to my experience, as recorded (Hajek[3]) in a paper read in 1902, recurrence of polypi may have one of the following possible causes:

(*a*) In the course of operation not all polypi are removed, and the individual polypi are not removed together with their base (Zuckerkandl[4]).

(*b*) In many cases the empyema, which gives the inflammatory basis for the formation of polypi, is overlooked. (Grünwald[5]).

(*c*) The bone, or the medulla, which with the polypi are intimately connected, is also diseased (Woakes[6]), and new polypi come out of the cellular infiltrate of the medullary spaces.

To these three causes I should like to add another one, which is the result of later observations:

(*d*) Continual polyposis of the nasal cavity can be produced by recurrences arising from the maxillary sinus (Hajek[7]).

Let us consider now the various possibilities and the therapeutic measures necessitated by them. It is clear that we cannot remove *in toto* with our usual methods all the polypi which originate from the depths of the middle nasal canal, from the hiatus, the infundibulum, or the depths of the upper nasal canal (Zuckerkandl). We only amputate them at various distances from their base. Very often a considerable portion of diseased mucosa remains, producing new polypi. But we see daily that recurrences stop if we resect the middle turbinate, and thus can approach the base of the polypi and remove them *in toto*.

Since Grünwald[5] has pointed out that an empyema of an accessory sinus is often the primary cause of polypi, we can observe every day that in many cases recurrences of polypi can be prevented by proper drainage or cure of empyema. Although I cannot accept Grünwald's opinion that the majority of polypi are caused by suppuration of accessory sinuses, nevertheless it cannot be disputed that in a large number of cases the existing suppuration of such accessory sinuses is a constant source of polypus formation. The third reason for continual recurrence is the diseased condition of the bone itself, which is present in inveterate cases of polyposis.

As regards the nature of this disease of the bones, various opinions are held by the authors. But it is

absolutely necessary to have one opinion, and the correct one too, if possible, for this controls our indications for operation; therefore I trust I shall be excused if I digress a little from my original task—treatment. There are two opinions opposed to each other as regards this problem. The one was formulated some time ago by Woakes,[6] who expressed his belief in the primary disease of the ethmoid bones. Zuckerkandl[8] and myself (Hajek[9]) do not agree with it, whilst the English surgeons—at least, as far as I can judge from the *Transactions* of the London Laryngological Society—still adhere to this opinion (Lambert Lack[10]), whilst other authors (Spencer,[11] H. Tilley,[12] StClair Thomson[13]) doubt the primary disease of the bones. Woakes makes a "necrosing ethmoiditis" responsible for the polyps, and his thesis has been proved, according to many authors, by Martin.[14] I wish to state emphatically that this is a mistake. Martin has only shown the existence of a rarefying ostitis, which has not been doubted by any of his successors, also not by Zuckerkandl[8] or myself.[9] But Martin already has strictly refused to accept the conclusions arrived at by Woakes—namely, that the necrosing "ethmoiditis" is the primary cause of polyp formation. Investigations by Zuckerkandl and myself, afterwards corroborated in the main points by Cordes[15] and Uffenorde,[1] have shown that the inflammation starts in the most superficial strata of the mucosa, that it goes deeper and deeper in older cases. Later on it affects periosteum, and causes partly the formation of new bone (osteophytes) in the periosteum, partly rarefying ostitis. My investigations, furthermore, have shown that the inflammation, spreading into the depths, also extends into the medullary spaces of the ethmoid bone, which is markedly spongy and fills them with cellular infiltration. The changes in the bone and the medullary spaces are therefore secondary, not primary, as Woakes and his pupils believe. The main consequence of these changes, recognized by all nasal surgeons, is the undoubtable fact that in inveterate polypus formation concomitant disease of the bone is the rule. There are present either periostitic irritations or inflammatory infiltration of the medullary spaces, which favour the return of polypi. It is therefore only logical to remove in such cases the soft tissues together with their bony basis.

The anatomical conditions of the ethmoid bone bring it about that the mucous covering of the nasal ethmoid bone extends without interruption into the interior of this bone, and that in *well-marked* polyp formation the interior covering participates just like the nasal mucosa in the production of polypi (myxomatosis interna—Bosworth). Even if the osseous base is not yet diseased, we must remove in such a case the osseous fundament of the ethmoid labyrinth, for it is not possible otherwise to remove thoroughly the polypi from the interior of the ethmoid cells.

I have observed[7] and published a unique case in which continually relapsing polypi originated in the maxillary sinus, whence they came into the nasal cavity through the ost. maxillare. The case was cured by opening widely the maxillary sinus and removing the mucosa of this sinus radically.

After this short explanation of the most important points of view, we will turn our attention to the methods of operation.

In the majority of cases the endonasal method with cocaine anaesthesia is the rule. Exceptions will be mentioned hereafter.

The bulk of the polyps is removed by means of a snare or forceps with a good grip. Polypi should be "evulsed" by the snare, for evulsion is much more effective than cutting with the snare, and should be always employed. Polypi with a broad base are best removed by evulsion with Luc's forceps.

I should like to mention that according to my experience a latent empyema of the ethmoid often becomes manifest through evulsion; for if the polypus, which blocks the openings of the ethmoid cells, is removed together with its attachment, the cells can now drain, and the empyema, formerly closed, is now open. Such observations prove that the methods adopted against an uncomplicated simple polyposis cannot be separated from those used in cases complicated by chronic suppuration.

Some surgeons recommend galvanic cauterization of the roots of the polypus after removal of the polypi in order to prevent a recurrence. This method cannot be endorsed—first, because it does not prevent a recurrence, and, secondly, the reaction set up by the cauterization can become excessive and produce dangerous complications. It is best to wait a few days after a polypus operation, until the reaction subsides, before another surgical interference takes place.

The operations are repeated until all visible polypi have been removed. We must remember that all procedures for the removal of polypi are only an introduction to the treatment proper, nothing more. Then we are enabled to examine repeatedly and thoroughly, and to answer those questions which are absolutely important for the correct judgement of the case—namely, whether the polypi represent the primary inflammation, or whether they are but secondary products caused by a previously hidden neoplasm, foreign body, or by a suppurating accessory sinus. The existence of this cause directs hereafter our therapeutic actions.

Repeated examinations by means of a very thorough cocainization of the nasal canals will show whether any nests of polypi are present in the middle or upper meatus. In the case of recurring polypi one can be sure that the polypoid degeneration is not restricted to the superficial mucosa, and that an extensive removal of the site of attachment of the polypi, often together with the osseous base, is indicated.

Mostly it is necessary to remove the middle turbinal in order to gain free access to the whole ethmoidal mucous lining. One is often surprised at the extensive degeneration of the mucous lining, which extends into the ethmoidal ostia and the depths of the cells of the infundibulum, and which one would not have expected before removal of the middle turbinated bone.

As regards the extent of the removal of ethmoid bone, with the exception of the lamina papyracea and the lamina cribrosa, each individual case must be considered separately. Also that part of the medial ethmoidal wall which is situated above the line of attachment of the middle turbinal should escape removal as far as possible, and should not be taken away high up. But one need not be too modest in removal of the uncinate process or of the septa of ethmoid cells and the degenerated mucous lining of the interior of the ethmoid bone.

As regards the method advisable for this operation, the following may be said: It is most important to cocainize well, care being taken to have adrenalin added. As much as possible should be removed in one sitting, absolute orientation during operation being a *conditio sine quâ non*. This last sentence I believe to be one of the most important leading ideas of any intranasal ethmoid operation. Further important conditions are: Quiet position of the patient owing to good cocainization; then absence of haemorrhage, which, starting early, seriously interferes with the view of the field of operation. Frequent use of the probe gives information as to the nature of the mucous lining of the ethmoid cells. I cannot consent to the demand that all should be removed at one sitting. Such a method, excluding all individualizing treatment, is fraught with danger to the patient. As long as one sees his way well, and the haemorrhage is small, and the patient feels well, one can continue to operate. If one of these conditions is absent we must stop and wait until the next sitting (Hajek[2]).

The obstacles which prevent our surgical interference based on the principles sketched just now may be of a twofold kind: (1) General; (2) local or individual. To the first group belong diseases which *per se* prohibit or restrict all surgical interference: haemoptysis, diabetes, chronic nephritis, etc., or diseases associated with general cachexia. A local obstacle may be given by insufficient accessibility due to a deviated septum or a spine. In that case the deviation must be corrected previously, for there is nothing more unsatisfactory than to grope in the dark or to manipulate in the region of the ethmoid with instruments and curettes without free access. An individual obstacle is, for instance, the liability to fits and collapse. Here the operation should be done under general anaesthesia, in spite of the increased difficulty. In cases of inveterate polypi repeated sittings are often required, as is well known, if one wants to remove thoroughly all diseased tissue. It is often necessary for a longer interval between the sittings, so that the reaction caused by one

operation may subside before the next is done. Such a treatment lasts, therefore, for weeks and months; yes, even after years slight recurrences may take place, but these are generally removed in a short time. But it is important to state that, although the treatment necessitates often many operations, and requires a good deal of patience and time both on the side of the surgeon and the patient, all cases can be cured finally, unless the above mentioned general reasons prevent continuation of treatment.

This tedious treatment of inveterate polypi explains why a more radical method, effected in one sitting, is sought after. As far as I know, the famous laryngologist, Lambert Lack,[10] a member of this honoured society, already ten years ago favoured such a method. But I do not believe that he has found many followers, for the following reasons: The operation is much more difficult under general than under local anaesthesia; the patient cannot aid us in clearing the nose; the most important point—the possibility of exact orientation mentioned before—is not guaranteed, for even a profuse haemorrhage cannot stop the operation, since all must be removed in one sitting. For the judgement of the merits of operation under general anaesthesia I would point out the following experience in several cases, where I had to operate for special personal reasons under chloroform: In spite of good anaesthesia, good anaemization of the ethmoid, the operation was not thorough, although I had removed all visible diseased parts. I several times removed afterwards oedematous remains of the diseased mucous lining of the ethmoid, which had escaped notice during the operation. But as one should strictly adhere to the principle of removing rather too little than too much, if orientation is not perfect, one has always to face the necessity of a later operation. In general anaesthesia one has the tendency to violate this principle and remove all disease at once if possible. This I believe to be a procedure that might make the operation a very dangerous one.

II.

We come now to the problem of chronic ethmoiditis, where the suppuration predominates in the clinical picture. Here we must at once differentiate between the (1) open and the (2) closed empyema, because the treatment is different in both kinds.

1. *Open Suppuration of the Ethmoid.*—Here we must, first of all, be guided by the diagnosis. I should like to say that the procedures required for an exact diagnosis form at the same time an important part of treatment. Diagnosis of open empyema of the ethmoid is based on the exclusion of empyema of other accessory sinuses, further on the free drainage of the ducts of the ethmoid cells, and on the finding by means of the probe. In order to fulfil all these requirements, one has mostly to remove all visible polypi and hypertrophied parts of the interior of the nose. Also the middle turbinate must be sacrificed mostly, because it is partly degenerated, and partly occludes the free view of the ethmoid labyrinth by its unusual curvature. But it must be conceded that at times the middle turbinate can be saved, and therefore we should aim at saving this organ, for it possesses a functionating cavernous body. Only after making free the middle meatus we are able to state whether an ethmoid suppuration is present alone or in combination with affections of other sinuses. Also the extent of the suppuration of the labyrinth can be recognized only after obtaining completely free access to the middle meatus and repeated use of the probe. These details as regards intensity and extent of the suppuration, found out by repeated examinations, must give us a clue to the kind and order of therapeutic interference. I am strictly opposed to a method which would put down a hard and fast line of treatment for all cases.

Let us suppose, for instance, we have to deal with an open suppuration of the anterior ethmoid cells—that is, those opening into the middle meatus—and that we have succeeded by means of the probe in finding out that the pus comes out on the roof of the middle meatus, between the bulla and the insertion of the middle turbinate—that is, one of the typical openings of the ethmoid cells. The curved probe goes about 1 cm. laterally towards the lamina papyracea. Nothing is easier and more rational than to open the ethmoid from the roof of the middle meatus through this ostium ethmoidale and thus to drain the cavities containing the pus. It is most convenient to use for this purpose my ethmoid hook to break down the bony walls around the ostium and then to enlarge the cavity by means of the ethmoid curettes mentioned before, biting away the surrounding bone. Then only can one see and consider the condition of the mucosa lining the interior of the cells—can see eventually a communication with other cells. If the disease has advanced further into the ethmoid, one can open the way into the diseased cells, remove their walls, and in a few minutes one can see nearly every point, control it with the probe, and get an excellent orientation regarding all details. The latter point is most important of all. As long as this is possible and the patient can stand it, the haemorrhage not hindering, one should remove as much as possible, and open up all pus cavities. It is not necessary to remove the walls of healthy cells.

In other cases the main suppuration is localized in the infundibular cells. The entire mucous lining of the hiatus semilunaris of the infundibulum and of the cells opening into the latter is diseased. In such cases it is the best plan to resect first of all the processus uncinatus, beginning at its anterior end at the attachment to the agger nasi. A free access to numerous infundibular cells is obtained by a resection of a part of the bulla. Not all authors are so careful as regards detailed diagnosis, but remove all accessible ethmoid cells. Thus, for instance, Stenger[16] regularly starts from the bulla and removes the labyrinth from here. It must be emphasized that the endonasal removal of the infundibular cells requires great caution in using the instruments, and one must carefully consider how far to go. The variable anatomic conditions of this region postulate restriction, for the orbit is endangered if one goes too far laterally, and if the infundibular cells reach high up the roof of the ethmoid, the lamina cribrosa may be injured. Here endonasal technique has its limit. Therefore, if infundibular cells reach high up, the extranasal route is indicated; this is the resection of the frontal process of the maxillary bone. But it must be stated that with the exception of the rare cases of infundibular cells reaching high up, all other parts of ethmoid labyrinth can be removed by the endonasal route perfectly well.

In suppuration of the posterior ethmoid labyrinth we can act in a twofold way: Either we begin with the operative removal of the anterior labyrinth, and then by gradual progression we enter the posterior part if the anterior and posterior labyrinth are diseased; or we open only the posterior labyrinth if the anterior is healthy. The preferable place for opening the posterior labyrinth is situated just above the line of attachment of the middle turbinate in the upper meatus (Hajek[2]). Here my ethmoid hook can easily be applied, and will open by pulling it in a forward, inward, and downward direction the posterior ethmoid labyrinth safely. If a part of the anterior labyrinth is also opened by this procedure, it does not matter much, for sometimes it must be done in any case for better orientation. But it is superfluous to do it *a priori* in all cases, as Stenger[16] does, starting from the bulla and working one's way through the whole labyrinth. The removal of the walls of the posterior labyrinth up to the anterior wall of the sphenoid sinus offers but little technical difficulty. Owing to the depth of the ethmoid labyrinth at this point an injury of the orbit is not at all likely, but one must be on the look-out for it when working on the infundibular cells in the hiatus. Such injuries of the orbit have been observed already repeatedly, and I have seen them, too, several times, but luckily without any further harm. The large amount of orbital fat is an excellent protection against the surgical interference of less experienced rhinologists.

Local obstacles to the accessibility to the ethmoid labyrinth are given by a deviated septum. In such cases one must not hesitate to correct the deviation before any extensive operation on the labyrinth is done, for an operative procedure in the region of the infundibular cells, in presence of septum deviation, is, according to my experience, a very bold undertaking.

The number of operative interferences depends not only upon the extent of the disease, but also upon the numerous variabilities as regards the resistance of the patient, the haemorrhage during operation, etc. Here one cannot lay down a hard and fast rule; each case must be judged according to its merits.

A very important question is that dealing with the intervals between the operative sittings. One should adhere to the rule never to operate until all the reaction caused by the previous operation has subsided. To adhere to this rule is, I believe, one of the most important points of after-treatment. If we operate before the reaction has subsided, then the next reaction is more intensive, and unpleasant complications are to be expected; furthermore, one must not forget that after an operation on the ethmoid also the healthy parts of this region are swollen and can appear like polyps, which, however, return to the normal state after a few days. This makes the differentiation between healthy and diseased parts much more difficult, sometimes even impossible.

As regards the instruments to be used, we can say that the instruments mentioned previously for the treatment of inveterate polypi are here also satisfactory (for example, the instruments of Schäffer,[17] Grünwald, Hartmann, Hajek,[2] etc.). The kind of instrument is indeed of secondary importance, as every author prefers his special favourite ones to which he is accustomed. Not the instruments are important, but the brain and the hand which apply them. One can be very liberal as regards the use of special instruments, but it is not right to describe a special method of operation of the ethmoid bone only on the ground that one prefers the blunt instruments to the sharp ones used by others (Stenger).

I have mentioned already that the extent of the operation to be done at one sitting is very variable. This depends upon the individual conditions mentioned several times. But the minimum which has to be effected must also be stated; for in breaking down suppurating ethmoid cells it is important that the opening be a large one. If this is insufficient, the reactive swelling may cause retention of the secretion, with a rise of temperature and threatening symptoms. I have published a case (Hajek[19]) in which the condition was extraordinarily obstinate, so that the operation done by one of my assistants was insufficient. We lost this patient through meningitis, caused by violent reaction and retention of secretion. The case has been published. If, in the case of an ethmoid operation, we are in doubt whether we shall be able, either owing to local endonasal difficulties or special individual conditions, to obtain sufficient drainage for the pus, then it is better not to operate endonasally, but to use the external route under general anaesthesia. The aim of after-treatment is, indeed, to prevent all retention. Therefore, if possible, packing should not be used. If this is not possible the packing should be removed as early as possible, or at least it should be applied as loosely as can be. Also intensive cauterization should preferably be not used during after-treatment. Only at the conclusion of the treatment may one apply solutions of silver nitrate (Hajek[2]). The definite healing or scarring of the mucous membranes of this region takes weeks, if not months. It is effected by shallow scar tissue, and never by filling of the ethmoid cells by granulations, as Stenger[16] says. In extensive ethmoid suppuration the final result is given by a cavity transformed after removal of the septa into a deep niche, which Bosworth[20] has for the first time postulated as desirable. It is also a good plan to soften the crusts, as long as after-treatment lasts, by means of oil and ointments. Sometimes it becomes necessary to promote healing a little with silver nitrate where the granulations on the edges of the injured bone threaten to become too exuberant.

2. *Extranasal Method.*—The extranasal method is indicated:

(1) In cases of suppurative ethmoid disease, when the endonasal method was not sufficient owing to the inaccessibility of the infundibular cells, also orbital cells, reaching up very high.

(2) In cases where the accessibility to the ethmoid is restricted owing to a narrow nose, and where threatening symptoms do not permit waiting until sufficient space is obtained by an operation for the anomaly of the septum.

(3) In such cases of ethmoid suppuration which are complicated with marked disease of the frontal sinus, thus being only part of the actual disease, and its operation taking place together with that of the frontal sinus.

(4) In such cases of ethmoid suppuration, where the endonasal method could not ward off a threatening bursting of the suppuration into the orbit, or where already bursting has taken place with formation of a circumscribed or diffuse abscess. In all these cases, only the external method can bring help.

In order to make the ethmoid labyrinth accessible from the outside, Kuhnt,[21] Grünwald,[5] and Jansen,[22] have worked out practical methods. Whilst Kuhnt and Grünwald lay open the ethmoid labyrinth by an incision through the skin, Jansen has forced his way into the ethmoid labyrinth through the maxillary sinus.

Of all the methods recommended, the incision devised by Killian,[23] with resection of the frontal process, is the best for most cases. Of course, in cases of formation of a fistula or abscess one does not always adhere to the typical operation, but has to regard the individual requirements of the case. Especially in cases of abscess formation all cosmetic considerations must be put aside; one must abstain from primary suture, and even the orbital bridge cannot be saved. For such cases the method devised originally by Riedel,[24] from which Killian's method differs only in so far as it saves the orbital bridge, can be stated to be the safest and most convenient. In cases with an extensive abscess in the orbit, especially in retrobulbary abscess, the access to the region one wants to operate cannot be large enough; one cannot pay attention to save the attachment of the trochlea; even parts of the lamina papyracea must be removed if they are necrotic.

Ethmoidectomy, recommended by Guiset,[25] will be hardly ever the typical operation in indicated cases of abscess alone, but *de facto* will approach it the more the lamina papyracea has to be sacrificed.

It is remarkable that even the radical methods do not guarantee a healing without recurrence. Also after the radical operation it will be necessary, just as after the endonasal method, to control the case for weeks, months, or years for the sake of removing now and then remaining or newly formed polypi. I have several times been called upon to remove endonasally most exuberant polypi after operations done half a year or a year before by fully competent nasal surgeons.

It must be finally stated that after operations of the ethmoid labyrinth fatal brain complications have occurred. Dreyfuss[26] and Gerber[27] have compiled these cases. Let us hope that strict adherence to the principles of operation and after-treatment mentioned above will make these sad cases less and less frequent.

3. *Closed Empyemata.*—The method of treatment of a closed ethmoid empyema has to adjust itself to the prevailing conditions of the case. This is readily understood if one considers that in such instances a certain diagnosis can be made only after operative interference. A cursory enumeration of the possibilities arising here will soon make it clear. I have mentioned already that at times after evulsion of a polypus situated high up pus wells up. The evulsion was succeeded by the diagnosis of the empyema, hitherto latent. Similar conditions prevail as regards the majority of circumscribed or diffuse dilatation of the ethmoid labyrinth. For instance, in the region of the middle turbinate or the middle meatus one sees a more or less marked bony enlargement, or tumour, the exact origin of which cannot be stated *a priori*. It can be either an enlarged middle turbinate or an ethmoid cell dilated by mucus or pus, or a bulla ethmoidalis, dilated by pathological contents, or the process uncinatus, enlarged by the same cause. The probe tells us that all these formations are cystic cavities with bony walls. Exact palpation shows a more or less distinct fluctuation here and there, which demands the opening of the cyst in that place.

If the cavity is opened, we can diagnose a suppuration in a bony cell, lined by a mucous membrane. This cannot belong to anything else but to the ethmoid labyrinth. The further consequence of our diagnosis is naturally a further removal of the osseous wall. The more we remove the bony wall, the clearer the topographic conditions will become, which, in their turn, will direct our further therapeutic interference. The endonasal opening of the closed ethmoid empyema is nearly always sufficient to effect a complete drainage. External opening is mostly not necessary, except that an extension into the orbit and abscess formation call for it urgently. The symptoms of dislocation of the bulbus which occurs in consequence of a closed empyema of the ethmoid disappear generally in a short time, so that the operation of a closed empyema is,

comparatively, one of the easiest and at the same time most gratifying of rhinological operations.

III.

The chronic suppurative inflammations of the ethmoid which appear under the symptoms of ozaena require a separate discussion, because they have a special position both from the points of view of pathological anatomy, of the clinic, and of treatment. It is not the task of this paper to consider the pathological, anatomical, and clinical positions of ozaena; therefore we shall not discuss the relation between affections of the accessory sinuses, especially the ethmoid empyema, and the concomitant ozaena so frequently met with. But in this paper it must be pointed out that the suppurative inflammation of the ethmoid bone must be treated on the same lines as the hyperplastic inflammation of the ethmoid bone, but that the indications for their operative treatment must be very much restricted in ozaena, as the therapeutic gain is mostly quite insignificant. According to my experience, only those cases of ozaena with chronic ethmoiditis should be operated on where the ethmoid labyrinth or part of it is the main source of the suppuration, and where we can therefore hope to improve or stop the bulk of suppuration by a change in the condition of the ethmoid. Only in cases of threatening complications of a closed empyema we may abstain from conservative methods.

IV.

The treatment of the inflammatory processes of the ethmoid accompanying tuberculous and syphilitic disease, as well as malign tumours, depends absolutely on the primary disease, therefore we need not discuss it here.

REFERENCES.

1 Uffenorde: *Die Erkrankungen des Siebbeines*, 1907.
2 Hajek: *Pathologie u. Therapie der entzündlichen Erkrankungen der Nebenhöhlen der Nase*. III Auflage, 1909.
3 Hajek: Warum recidiviren die Nasenpolypen, *Vortrag*, 1902, *Wien. med. Presse*, 1902.
4 Zuckerkandl: *Normale u. pathologische Anatomie der Nasenhöhle*, Bd. I, II Auflage, 1893.
5 Grünwald: *Die Lehre von den Naseneiterungen*, München, 1893.
6 Woakes: Necrosing ethmoiditis, *Lancet*, 1885, 18 July.
7 Hajek: Ein Beitrag zu Rezidive der Nasenpolypen, *Archiv f. Laryngol.*, 17 Bd., III Heft.
8 Zuckerkandl: *Normale u. pathologische Anatomie der Nase*, Bd. II, 1892, I Auflage.
9 Hajek: Ueber die pathologischen Veränderungen des Siebbeinknochens im Gefolge der entzündlichen Schleimhauthyperthrophien u. der Nasenpolypen, *Archiv f. Laryngol.*. Bd. IV, Heft III.
10 Lambert Lack: Laryng. Society of London, 1900; *Centralbl. für Laryngol.*, 1901.
11 Spencer: Ibid.
12 H. Tilley: Ibid.
13 StClair Thomson: Ibid.
14 Martin: BRITISH MEDICAL JOURNAL, 1892, December 24th.
15 Cordes: Ueber dio Hyperplasie, die polypi die Degeneration der mittl. Muschel der Nasenpolypen u. ihre Beziehungen zum knöchernen Teile des Siebbeines, *Archiv f. Laryngol.*, Bd. XI.
16 Stenger: Zur Technik der endonasalen Siebbeinoperatton, *Zeitschrift für Ohrenheilkunde u. f. d. Krankheiten der Luftwege*, lxiv, Bd. III u. IV, 1912.
17 Schäffer: *Chirurgische Erfahrungen in der Rhinologie u. Laryngologie*, Wiesbaden, 1885.
18 Hartman: *Atlas der Anatomie Stirnhöhle*, etc., 1900.
19 Hajek: Ein Beitrag zum Studium des Infektionsweges bei rhinogenen Gehirnkomplikationen, *Arch. f. Laryngol.*, 1906, Bd. XVIII.
20 Bosworth: *Archiv f. Laryngologie*, Bd. III. u. IV.
21 Kuhnt: *Die entzündlichen Erkrankungen der Stirnhöhle*, Wiesbaden, 1905.
22 Jansen: Berichte des Moskauer Congresses, 1897.
23 Killian: *Archiv f. Laryngol.*, Bd. I, S. 320.
24 Riedel: Publiziert durch Schenke, Dissertation, Jena, 1898.
25 Guiset: Huit cas de trépanation du système sphéno-ethmoidale par voie orbitaire, Extrait des Bulletins et Memoires, Congrés de 1906, Paris.
26 Dreyfuss: *Die Krankheiten des Gehirnes und seiner Adnexa im Gefolge von Naseneiterungen*, Jena, 1896.
27 Gerber: *Die Komplikationen der Stirnhöhlenentzündungen*, 1909, Verlag von S. Karger.

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THE treatment of suppuration in the ethmoidal cells may be operative or non-operative, and the former may be conducted entirely through the nose, or involve an external wound. In my opinion, operative measures are invariably indicated, and other measures—even the fashionable vaccine therapy—are useful only as adjuncts. Suppuration in the ethmoidal cells is almost always accompanied by polypus, and frequently by suppuration in other accessory sinuses; also orbital or other external complications may be present. It is probably more often productive of serious symptoms than suppuration in any of the other sinuses. These symptoms are such as to necessitate treatment for their relief, even though it is not necessary, nor in all cases advisable, to insist on absolute cure of the disease.

Piecemeal Operations.—The methods of piecemeal operations through the nose under local anaesthesia I need hardly discuss, as I consider them both inadequate and dangerous. They belong to the older days of rhinology, before surgical experience had been brought to bear upon the subject, when the rhinologist was still too timid to employ general anaesthesia in operations involving bleeding in the nose, and was afraid of touching the ethmoidal region unless he had the patient sitting upright and could "see" exactly what he was doing. These "operations" required constant repetition; weekly or almost daily treatment, extending over two or three years, was necessary to produce even a "half cure." Patients would rarely submit to this, and the few who did were usually reduced to a condition of profound neurasthenia. Fewer still would admit that their nasal symptoms were improved, whatever might be the view of their enthusiastic physician. I am speaking only of extensive disease; when only one cell, such as the cell in the anterior end of the middle turbinate, is involved, a trivial operation with local anaesthesia is sufficient, but then only a single sitting is required.

Ethmoidal Curettement.—This operation still appears the best for most cases of ethmoidal suppuration. Take a typical case presenting more or less the following symptoms: There is nasal obstruction, purulent discharge, headache, neuralgia, or various aching pains, often associated with mental depression and other neurasthenic symptoms. One, often both, sides of the nose are stuffed with polypus, or the upper part of the nose is filled with small oedematous granulations; between the polypi and granulations pus exudes. Sometimes the pus may not be apparent at first, and the operator may commence by snaring the polypi, and only then discover the extensive disease in the ethmoidal region. In such a case, when it is quite certain that there is extensive ethmoidal disease, with suppuration in many cells and polypoid degeneration of their lining membrane, possibly also suppuration in other accessory sinuses, the operation which I have described as "ethmoidal curetting" should be carried out. It is well to pack the nose half to three-quarters of an hour before the operation with a weak solution of cocaine and adrenalin, as it permits a better view of the parts, and the danger of reactionary haemorrhage appears to be remote. This also facilitates exploration of the antrum. A general anaesthetic should be given, and the patient should lie on his right side with the head slightly raised and turned to face the operator, who sits on a stool by the side of the operating table and uses a reflected light, as in ordinary rhinoscopy.

If there is any suspicion of antrum suppuration, the first step should be to explore that cavity, and if pus is obtained, it is best to deal with it at once. In these cases I consider a radical operation on the antrum is necessary, and therefore proceed to remove the anterior end of the inferior turbinate and the whole inner wall of the antrum. The attention is then turned to the ethmoidal region. Any large polypi should be removed with forceps, Luc's forceps being probably the best, and the middle turbinate, if present, clipped away with Grünwald's forceps. Directly this has been done and the ethmoidal cell region exposed, the ring knife should be introduced. As I have said previously and would like again to emphasize, the curette should be introduced gently with the blunt edge of the ring towards the septum, and passed as far upwards and backwards as possible without using force. The blade is then sharply dragged horizontally forwards along the ethmoidal region with the sharp edge directed *outwards* towards the orbit. On no account should the cutting edge of the blade be turned upwards towards the cribriform plate. The scraping is continued rapidly and with a fair amount of force until all friable tissue is removed. A great deal of polypoid mucous membrane and bony débris is usually scraped away, and almost the whole of the lateral body of the ethmoid can be thus removed. Often it will be found that a large communication has been made into the upper part of the antrum, and frequently the inner wall of the orbit has been broken down and the orbital periosteum exposed.

Other details of this operation I have frequently described, and will pass on to consider the results. I

have taken some trouble, and have given others considerable trouble, to ascertain the results of this operation, not by the common but fallacious way of consulting published reports, but by circularizing some of my busiest and best friends. I am greatly indebted to them for their kindness in replying. Many of them have carefully looked up the notes of their cases, and have supplied me with valuable statistics and suggestions. In particular, I am indebted to Dr. J. W. Bond, Mr. Charles A. Parker, Mr. Ernest Waggett, Mr. Fitzgerald Powell, Mr. Gay French, Mr. Charles Heath, Mr. Herbert Tilley, Mr. G. Constable Hayes, Dr. Bronner, Mr. E. W. Bain, and Mr. Frank Rose. From grouping their reports and from the notes of my own cases, I think some definite conclusions can be formed.

As regards the ultimate condition of the nose, the results of the operation are excellent. The removal of such an extensive portion of the bones and mucous membrane rarely gives rise to any ultimate inconvenience to the patient. The upper portion of the nose becomes lined with a healthy white scar membrane, and only occasionally is there any undue dryness or crusting in the nose or post-nasal space. So far from destroying the olfactory sense, the patient often regains the power of taste and smell which he may have lost for years. In all cases the result is not so successful as this. There may be a continuance of suppuration or some remairs of polypoid degeneration. Many reports state that subsequent trimming away of tags under cocaine anaesthesia has been necessary. It must be borne in mind that a continuance of the suppuration may mean disease in other sinuses. The clearing away of the ethmoidal cells facilitates the diagnosis of this, for when the antrum has been explored and the ethmoidal region thoroughly curetted, pus coming down from high up in the front of the nose can only come from the frontal sinus, whilst pus at the back of the nose must come from the sphenoidal sinus, the ostium of which has been freely exposed to view by the removal of the middle turbinate and the posterior ethmoidal cells. In this connexion one must also remember that the antrum, even if healthy before the operation, may become infected at or soon afterwards. In other cases where the disease has been very extensive and the operation for one reason or another not carried out so thoroughly as it might have been, a *partial success* may be obtained, and the patient's symptoms, such as pain, headache, and the sense of nasal obstruction, may be relieved, although the appearance of the nose may be anything but satisfactory. Lastly, there may be *adhesions*. These are due to injury of the septum during the performance of the operation, and they may usually be avoided by care at the operation and in the after-treatment. The use of cocaine and adrenalin as above described diminishes this risk. *The dangers of the operation* I wish to consider fully.

1. *Mortality.*—In my own practice, in over 300 cases, I have, unfortunately, had one death. This was due to neglect of the precautions which I have above emphasized. The case was one of chronic ethmoidal suppuration associated with asthma; the patient took the anaesthetic badly, was somewhat congested, and bled freely, so that it was impossible to see what was done. The middle turbinate and ethmoid seemed unusually dense, and the curette produced very little effect. Too much force was used, and the blade of the instrument was turned upwards. The patient developed meningitis, and died a week later. In the reports which I have had from eleven different operators, eight state that they have had no deaths; the other three report six deaths. Parenthetically, I may remark that these figures illustrate the advantage of the method of investigation, for there are no deaths recorded in the published accounts of the operation. From a study of the details I should say most of the deaths were avoidable. Two cases, both young girls, did well until the fifth day. They were in wards which were unusually septic at the time, and one was dressed by a house-surgeon suffering from a septic throat. Two of the others were old people over 60, their noses were widely distended with polypi, and they had had growths removed for over thirty years. One of these cases apparently died from traumatism, a cribriform plate which was very brittle being fractured; the other died of sepsis. All the deaths were due to meningitis; I have heard of no death from any other cause. In considering these results it must be borne in mind that the operation is a serious one, but it should only be carried out to cure an undoubtedly serious and incapacitating disease, and therefore some risk is justifiable. Also remember that these statistics date from the earliest days of the operation before any experience had been gained, and whilst some surgeons considered it an absolutely safe procedure, and therefore neglected the precautions which we now know to be necessary. I quite agree with the opinion expressed by Dr. Bond, one of our most cautious and level-minded colleagues, who has had large experience in this operation. He considers that there is little danger (1) if the patient is not aged, (2) if the operation is done skilfully, and (3) if the operation and after-treatment are carried out with full aseptic precautions and the patient's surroundings are free from sepsis. With regard to age, I used to confine the operation to patients under 40 years, and would still hesitate to operate on people over 50. Between 40 and 50 each case may be judged on its merits; there can be no hard and fast age limit. As age increases the ethmoid becomes more brittle, and there is more danger of fracture of the cribriform plate, and the patient is less able to stand the loss of blood, the shock of the operation, and the complications arising from blood entering the air passages.

With these precautions I think the risk could be reduced to less than 5 per 1,000; it would certainly not be a tenth of the mortality of operations on the frontal sinus. There is no alternative method giving anything like such good results with so small risk.

2. *Septic Troubles.*—In some of my early cases there was considerable fever two or three days after the operation. This rise of temperature almost certainly arose from packing the nose. For many years I have given up this practice, and the after-treatment has consisted in gently irrigating the nose daily with a mild, unirritating lotion, such as warm boric acid solution. Since then I have had no trouble with fever. Other operators have been led to abandon packing for the same reasons. Some advocate the use of vaccines preparatory to the operation, and think the absence of fever subsequently may be traced to this. These views are open to discussion: I am not convinced of their correctness, and would not, with our present knowledge, advocate vaccine treatment.

3. *Haemorrhage.*—Bleeding is often profuse at the time, but rapidly ceases. Only once or twice has it been necessary to pack the nose for this reason. Should the bleeding really be severe and not yield to simple remedies, such as the application of cold to the face, then the nose must be packed, and I would recommend the introduction of strips of gauze into the ethmoidal region. These should be removed twelve to twenty-four hours later, and not replaced unless again required.

4. *Orbital Complications.*—When the orbital plate has been removed or fractured and the periosteum exposed there may follow some effusion into the orbit or a severe "black eye" may be produced, occasionally with some pain or impaired mobility of the globe. In two cases where ethmoidal disease was associated with antrum suppuration an abscess formed in the lower eyelid. All the other cases of "black eye" have passed off in a few days without subsequent trouble.

5. In one case privately reported to me and published in my book, and in another published by Robinson, of Brisbane, optic neuritis and blindness have followed the operation. It seemed doubtful if the operation itself was the direct cause of the trouble; in one case the operator ascribed it to the rapid loss of blood.

No other ill results have occurred, so far as I know, in my own practice or in that of others. A consideration of these results shows the necessity for care and skill in performing the operation and in avoiding unsuitable cases. The operation remains, in my opinion, the best method of dealing with uncomplicated cases of chronic ethmoidal suppuration.

Finally, the *various external operations* and their indications. The latter depend upon the complications that are present. When there is definite evidence that the case is complicated by frontal sinus suppuration with such symptoms as render an operation on the frontal sinus advisable, then I would recommend that the ethmoid be treated at the same time and through the same incision. The skin wound is extended a little lower down and the bone freely cut away to expose the infundibular region, when the anterior ethmoidal cells come into view and

may be freely cut away or curetted. The ring knife again seems to me the safest and most effective instrument to use, and it may be inserted through the wound and guided by the finger in the nose, or vice versa. It is hardly correct to state that it is possible even by this method to see what is being done. The bleeding and the small external opening render the view obtained of little use.

Watson-Williams's operation on the frontal sinus gives an excellent approach to the anterior ethmoidal region. Although I have little experience of this method in cases of suppuration I have often performed it in malignant disease, and can testify to the excellent way in which the ethmoidal region is exposed. By this operation, more than by any other with which I am acquainted, can the ethmoidal region be brought into view and a thorough dissection made right through from front to back; even the sphenoidal sinus could be opened if necessary.

An external operation should also be performed when any orbital complications are present. I think in every case in which there is a fistula, or an abscess, or any swelling in the orbit, possibly or probably of ethmoidal origin, it is far safer to operate through the orbit than to attempt an operation through the nose. The incision is made around the inner angle of the orbit, similar to the incision for the frontal sinus but a little lower, and carried down to the bone. The periosteum over the inner wall of the orbit is separated from the lacrymal plate, and the sinus or abscess is thus exposed and traced to its origin. The ethmoidal cells are thus freely opened up as far back as necessary. The operation must be completed by making a free communication between the orbit and the nose, so that the subsequent drainage can be carried out from the nose. Orbital drainage alone must never be relied on.

The results of these external operations are exceedingly good. So far I have had no deaths nor any serious complication, but they are more serious than ethmoidal curettement. They involve more disturbance to the contents of the orbit, more shock, and a more prolonged convalescence. I certainly would not recommend them as the routine method of operation in chronic ethmoidal suppuration, but would reserve them for cases in which orbital or other complications are present. I hope other speakers, and especially those who have had more experience of Watson-Williams's excellent method, will give us their views on this subject.

With reference to *non-operative methods*, a very few words will suffice. Vaccine therapy is the only treatment which has been recently brought forward as an alternative to operation. Although I have had little experience of it, that experience has been so unfavourable that I regard the treatment as useless. My experience has been limited to cases in which an operation has failed to give a wholly successful result. When, after the cavities have been opened up—and in this connexion I may speak of antrum and sphenoidal as well as ethmoidal suppuration—a purulent or muco-purulent discharge has persisted, I have referred the patients to various pathologists in order that cultures may be made of the organisms present and vaccines prepared. As in all these cases reasonably free drainage had been established, they might be looked on as very favourable for vaccine treatment, yet I cannot honestly say that in any single case have I seen definite improvement, although the treatment has been continued for months, even years.

Lastly, it has also been recommended that the patient be prepared for operation by a course of vaccine treatment, but if the vaccine cannot cure a case in which there is but a little remaining discharge, it is not likely to do much good where no drainage has been established. I doubt if it would diminish the risk of operation, but upon this point other views may be held.

Personally, I should prefer to rely on carrying out these operations under the most favourable hygienic circumstances, and with full antiseptic precautions; and after the operation, wherever possible, the patient should be sent into fresh, pure air to recuperate. There is no doubt that all cases of nasal polypus and suppuration heal much more rapidly if they can be sent into fresh, especially seaside, air, than they do in the dirty wet atmosphere of a town.

To sum up, there are three methods of dealing with chronic ethmoidal suppuration, each of which has its advantages and indications, and also its disadvantages.

1. When there is frontal sinus suppuration, or orbital, or other complications, an *external* operation seems to me always advisable.
2. In all other cases where the patient is otherwise in good health and is not too old, ethmoidal curetting should be carried out. In some of these cases one should be content with removing the patient's serious symptoms, although signs of suppuration may continue.
3. In the aged and weakly one must be content to keep the patient as comfortable as possible by an occasional operation under local anaesthesia.

DISCUSSION.

Dr. WILLIAM BALLENGER (Chicago) said he would not defend his technique other than to say that in his hands it had proved eminently satisfactory. There must be a reason why some cases were acute and some chronic. The former rarely had any obstruction in the region of the middle turbinate, but this condition existed in practically all the chronic cases. The middle turbinate might be enlarged in a variety of ways, the septum deviated, etc. In acute disease there was a transitory lowering of the vitality of the mucous membranes of the body. This he considered a predisposing cause. He quoted Ziegler as saying that any cavity lined by mucous membrane, and that had its drainage partly closed, was prone to infection and inflammation. The speaker recognized that there were cases requiring only partial operation, but he would deal with the more complete methods. He was led to his method by noting that there were three attachments of the ethmoid—sphenoidal, cranial, and orbital—and that after making cranial and orbital incisions, the ethmoid could be removed *en masse*. Good drainage gave the best results, and avoided bad sequelae, and he had noticed that when he operated upon the old lines of partial removal there was considerable swelling and obstruction to drainage afterwards, but since he had adopted the more radical method this had never occurred. He could not explain his technique, but he would do his best to illustrate his procedure by diagrams, which he drew upon the board as required. He had had only one death since he adopted his radical method, and that was due to meningitis, which had evidently been present before operation, and then became acute. Emphysema of the orbit had twice occurred, and disappeared in twenty-four hours. He had never had orbital abscess or cellulitis, or any other complication of any moment. It was important not to pack. Anterior or posterior ethmoidal arteries did not give any trouble, but a branch of the middle meningeal might do so. He operated under local anaesthesia, allowed the patients to wait a little for possible haemorrhage, and then sent them home, and had done over 200 cases without being called to check haemorrhage. The after-treatment was done at his office. It consisted of a pledget soaked in a 10 per cent. aqueous solution of ichthyol and placed for twenty minutes in the region operated upon. It was then removed, and the patient went home. He was doubtful as to the value of vaccine treatment.

Dr. MOSHER (New York) said: Of all intranasal operating, manipulations carried out in the ethmoidal labyrinth have been to me the most unsatisfactory and disquieting. This condition of affairs has persisted in spite of abundant opportunities, of which I have freely availed myself, to make use of a liberal supply of anatomical material. Being in this unfortunate frame of mind, I have preferred to do all extensive ethmoidal operating by the external route. During the last twelve months, however, it has been my good fortune to have the applied anatomy of the anterior part of the ethmoidal labyrinth emerge, in a measure, from the mist which had so obstinately shrouded it. As a result, I have returned to intranasal ethmoidal operating with an increase of confidence which I had despaired of attaining. The following is a short summary of a paper on Intranasal Surgery of the Ethmoidal Labyrinth, which I presented at the last meeting of the American Laryngological Association. The

article has not been published, so I hope that my reading this extract here will not be looked upon by you as a discourtesy, especially when I add that I feel the responsibility of speaking before this distinguished body of men keenly, and hesitate to trust myself to speaking without notes. The summary is as follows:

The anterior boundary of the ethmoidal labyrinth is made by the internal angular process of the frontal bone and the posterior surface of the ascending process of the superior maxilla. The labyrinth cannot be entered effectively unless the curette is carried outward behind the ascending process of the superior maxilla, toward the lacrymal bone. Removing the anterior end of the middle turbinate and curetting upward and not outward does not open the labyrinth to any extent. The internal angular process of the frontal bone makes two-thirds, or the whole of the bony ring, which is the first part of the naso-frontal duct. The best guide to the duct is the posterior surface of ascending process of the superior maxilla. In a large number of cases the naso-frontal duct is not a tubular canal, but consists rather of a bony ring at the beginning and then becomes a triangular antro-posterior slit. This is more like an ethmoidal cell or an irregular meatus than a duct. The nasal frontal duct tends to run from without inward and to come into relationship with the anterior end of the extreme upper parts of the middle turbinate. When the drainage canal has the cell form, the anterior end of the middle turbinate makes its inner boundary. The duct, therefore, is reached most easily through the nose and through the anterior end of middle turbinate in line with the superior turbinate. A curette introduced at this point and carried outward toward the lacrymal bone and then withdrawn a little and carried straight downward and backward enters the anterior part of the labyrinth behind the ascending process of the superior maxilla and breaks down the cells through which the naso-frontal duct runs, destroying both the cells and the duct. Very little curetting is required to convert the anterior part of the labyrinth into a single cavity. In the roof of the chamber, secured by these manipulations, and usually in the anterior outer angle, the opening of the naso-frontal duct is placed.

If it is the wish of the operator to clean out all the ethmoidal cells, the posterior half of the labyrinth is entered by piercing the attachment of the middle turbinate and by curetting still further backward, using all the while the outer side of the middle turbinate as a guide. If the head of the patient is held level, the middle turbinate guides the curette into the posterior ethmoidal cell. Often the posterior half of the labyrinth is a large cavity made up of only one or two cells. This portion of the labyrinth has been, as it were, exenterated by nature. When the curette brings up against the back wall of the labyrinth, the remaining part of the middle turbinate and the lower half of the superior turbinate are removed. Then the posterior part of the superior turbinate is taken away flush with the front face of the sphenoidal sinus. The operator now recognizes the inner part of the front face of the sphenoidal sinus, which is free in the nasal cavity, and the outer part, which has a common wall with the posterior ethmoidal cell. The posterior upper outer angle of the posterior ethmoidal cell is dangerous to curette or to probe. In this locality it is of the utmost importance that the operator should be sure of his landmarks. He orientates himself by finding the superior rim of the choana and by differentiating the free face of the sphenoidal sinus by proceeding upward from the rim of the choana close to the septum. Having made out the extent of the free face of the sinus, the width of the common wall between the sphenoidal sinus and the posterior ethmoidal cell is determined. The dividing line between the two parts into which the anterior face of the sphenoidal sinus is directed is made by the obliquely vertical attachment of the superior turbinate.

The commonest mistake made by the operator is to get lost in the posterior ethmoidal cell. This comes about by curetting too high and too far outward, and by considering the posterior wall of the posterior ethmoidal cell as the whole of the front face of the sphenoidal sinus instead of the outer half of it. This mistake, if persisted in, will carry his instrument into the brain. Insufficient removal of the posterior part of the superior turbinate, and allowing the head of the patient to become tipped upward, are the chief causes of this confusion. After the landmarks of the front face of the sphenoidal sinus have been cleared and recognized the sinus is entered near the septum, if possible through the ostium, and the whole of the anterior wall removed.

The mishaps of the operation are entering the orbit through the lacrymal bone and entering the posterior part of the anterior fossa of the cranial cavity at the apex of the orbit. The first accident is trivial, the second is fatal.

Puncture of the anterior end of the middle turbinate on a level with the superior turbinate for catheterizing the frontal sinus or exenteration of the anterior ethmoidal cells is readily accomplished under cocaine anaesthesia. For the complete removal of the anterior and posterior cells, especially if this is to be accomplished at one sitting, it is more satisfactory to use a general anaesthetic.

Dr. Watson-Williams (Bristol) was convinced that ethmoiditis and resulting polypus formation was an infective process; this view he urged years ago, both in this Section of the Association and more recently at the Royal Society of Medicine, when he endeavoured to prove that the infection first involved the mucosa, and, when it extended to the periosteum, rarefying and formative osteitis occurred. The process was not primary affection of the bone, but of the mucosa, and the polypi were probably the result of a localized obstruction of lymphatic vessels, leading to an ever-increasing localized oedematous infiltration. The process of polypus formation extended over a long period, and in the earlier stages there might be no macroscopic appearances indicative of what was occurring, the symptoms being restricted to sneezing and rhinorrhoea. Hence the desirability of caution in prognosis; one never could tell the extent of processes which were only in the developmental period. In the same way polypi formed in the antrum or frontal sinus, and in the Zuckerkandl collection at Vienna examples of a frontal sinus and of a sphenoidal sinus polypus extending down into the nose were shown, and a few days earlier he had had a case of an enormous firm post-nasal polypus which grew out through an accessory maxillary ostium. Once an ethmoiditis became established as a chronic pathological process, operative procedures to ensure free drainage were essential to a cure. Personally he preferred cutting forceps to the curette, moderately large for the lower ethmoidal cells, but small and very light for opening up the higher cells, whether posterior or anterior, and particularly for the infundibular. Those that he showed he had found most suitable and safe. He went very cautiously here. Moreover, he avoided too free a removal of the vertical plate of the middle turbinated body. Only under exceptional circumstances were external operations necessary, except when the ethmoidal disease was complicated by frontal sinus involvement, and with the small forceps he had shown it was often possible to free the fronto-nasal duct sufficiently to allow of lavage and drainage. He was very glad to hear from Dr. Lambert Lack that he had found his osteoplastic method so useful, and that it afforded a better exposure than any other available procedure. He did it relatively seldom, and only when the necessity of a radical operation was imperative, but he thought it was safer to see what one was doing when one had to deal surgically with the higher fronto-nasal ethmoidal cells. Finally, as regards the use of vaccines, his experience had not encouraged the belief that they would avoid the necessity for operation, however desirable they might be after operation. But in sinusitis due to, or associated with, streptococcic infection he thought the previous injection of full doses of anti-streptococcic serum tended to prevent septic infective processes due to the operation itself.

Mr. Herbert Tilley (London) thought that all would agree that the principle underlying the treatment of chronic ethmoid inflammation was removal of the main disease and the establishment of free and permanent drainage. Opinions only differed as to the best method of carrying out the latter. Professor Hajek, as characteristic of the Austrian and German speaking race, preferred as a general rule to proceed slowly, with great thoroughness and minute attention to detail, and since the majority of his patients were also of this type of mind and nurtured in discipline, he attained his goal, and no one would dispute how excellent were the results. In England, however, both the patient and the surgeon preferred to get rid of the trouble as quickly as possible, and few of the ordinary English patients, hospital or private, would tolerate numerous sittings for the gradual removal of ethmoidal disease, and the time involved in the treatment. Hence the speaker preferred Dr. Lack's method—namely, a preliminary application of cocaine and adrenalin to the affected region, followed by a general anaesthetic. The diseased ethmoidal cells could be quickly removed; haemorrhage could be checked by strips of gauze moistened in hydrogen peroxide solution or adrenalin. He preferred ethmoidal forceps to the curette, but that was merely a personal predilection, and did not affect the principle of the operation. Much information could be gained as to the progress one was making during the operation by insertion of the little finger, and one assumed that the surgeon would be wearing rubber gloves. Soft or diseased structures felt very different from the hard, resistant, normal ethmoidal cells. When very free access was desired, the speaker would prefer to enter the nasal

cavity under the upper lip, and remove the ascending process of the malar bone (Denker's method) rather than to make an external opening. The first method gave admirable access to the ethmoidal mass, and left no visible scar. When chronic ethmoidal suppuration was combined with frontal sinus suppuration, he thought that it was daily becoming more feasible to relieve and cure these cases by intranasal operation, that is, removal of the diseased ethmoidal cells, enlargement of the fronto-nasal canal, followed by irrigation and ultimate destruction of the degenerate portion of the lining mucous membrane of the frontal sinus by the use of strong solutions of silver nitrate.

Mr. JOBSON HORNE (London) said that whilst congratulating the Section upon the happy solution to the ever-increasing difficulty in finding fresh subjects for discussion, he thought the title might lead to a misconception. For their own purposes the title was useful, inasmuch as it defined the scope of the discussion, but in the minds of others it might create the impression that chronic suppurative ethmoiditis was an entity. It was quite evident from the opening papers that the sphenoidal or the frontal sinus, or both, were not uncommonly involved. It was as well, therefore, not only on anatomical and developmental but also on clinical grounds, to regard the frontal and sphenoidal sinuses as more highly developed parts of the ethmoidal labyrinth. He urged the use of x rays in the investigation of the extent of the disease and of the presence or absence as well as of the size and situation of the larger cells. With such anatomical vagaries it was impossible to define any routine treatment; one had to treat the individual case. He had not been convinced by the arguments put forward in favour of extreme measures being immediately adopted. Personally he was opposed to operations in the nasal regions which were not carried out under ocular inspection. He ventured to ask whether any one of those present would personally submit to curettement or to the exenteration of the ethmoidal labyrinth prior to the adoption of milder measures.

MR. WESTMACOTT (Manchester) always used the method of operation by the intranasal route described by Dr. Hajek, and never in any chronic disease had it been necessary to do the external operation. He used general anaesthesia, and removed the whole middle turbinal, and then broke down the septa of the labyrinth with the hook and punch forceps. The nose was packed with gauze steeped in liquor hamamelidis (*B.P.*) for one to four or five hours, and then removed without renewal. Daily lavage and applications of protargol-argyrol, or iodine tincture, and occasionally pure carbolic acid. He would like to know how Dr. Hajek dealt with obstruction by the septum; further, could he explain the occurrence of a neuralgia in the nasal process of the superior maxilla which was occasionally met with? It was very persistent. He agreed with Mr. Tilley that the usual British patient would not have patience for slow methods. He had also found the sense of smell return after operation and not in others. Why was this? Was it ascending degeneration of the nerve or mechanical pressure causing death of the nerve? In other cases, even with complete filling of the nasal cavity with hypertrophies, there was no anosmia.

Dr. BRONNER (Bradford) was in favour of using large flat forceps, removing as many of the diseased cells as possible under direct examination, and to try and get permanent free drainage. Lack's operation was useful in some cases, especially when the patient could not afford the time to be examined and treated frequently. It was not fair to describe it as the operation of the future. He hoped that at the International Medical Congress next year Dr. Ballenger would demonstrate the operation he had described.

Dr. WILLIAM HILL remarked that there were probably few present who had performed a considerable number of ethmoidal operations of magnitude who could claim a clean record as regards mortality. Dr. Lack had not shirked this question of slight and perhaps unavoidable risk. He was, however, not quite able to follow Dr. Lack's line of reasoning when he advised that patients over 50 with ethmoidal disease should be left severely alone as far as any extensive exenteration was concerned, holding that surgical measures in such cases were literally "surgical interference," and more dangerous than the neglected disease. Seeing that fatal complications were usually due to stirring up an infected area with resulting septic meningitis, and not to shock and excessive bleeding, which two latter might be urged as a contraindication to operation in those past middle age, he doubted if the slight mortality from intracranial septic infection would be found any greater in the old than in the young; if the former might be well advised to put up with the disease in spite of its depressing effect on the patient's general well-being, on what grounds could young adults be more appropriately urged to run the slight operative risk? As far as he could gather, the danger of subsequent fatal septic meningitis bore no relation to the magnitude of the operation or to the microbic findings. One of his hospital patients had died from diffuse meningitis after so slight an operation as removal of the anterior extremity of the middle turbinal. It was true that there were two other unexpected deaths from sepsis in the same ward during the same week. His (the speaker's) remarks concerning the apparent want of proportion between the magnitude of the operation and the risk of fatal intracranial sepsis were not intended to apply to operative mishaps where the cranial cavity was accidentally opened up. He had heard much of the danger of injuring the cribriform plate; as a matter of fact, that structure was protected by its high position above the extremely narrow highest meatus and was extremely difficult to get at if fairly large-sized forceps and curettes were used, and this was an argument against the small cutting forceps advocated by Dr. Watson-Williams. Although he (the speaker) usually relied on the method of operating favoured by Dr. Lack, yet he was bound to admit that since he had made a pilgrimage to Bristol to watch Dr. Watson-Williams perform his osteoplastic operation he realized that that ingenious procedure was the ideal one to employ in those severe cases of pansinusitis in which there was not only extensive ethmoidal disease, but there was also involvement of the frontal and sphenoidal sinuses. By no other method that he knew of could such an extensive exenteration of the diseased sinuses be accomplished. One was not only comparatively close to one's work but actually in touch with it, for even the more posterior ethmoidal cells and the sphenoidal sinus could be opened up under the combined guidance of the eye and finger; and it appeared to him much easier by Watson-Williams's operation to avoid getting out of bounds—for example, entering the orbit or cranial cavity. It had been a great treat to him to listen to Dr. Ballenger's lucid exposition of his particular method aided by the excellent blackboard illustrations. The essential drawback to an operation performed from behind forwards was that it was a blind method, and its successful performance depended on the sense of touch and accurate direction; moreover, in using a single instrument, cutting not only vertically but also transversely, it had to be borne in mind that, whilst the posterior cells were wide transversely, the anterior ones were often extremely narrow, and there was risk of opening the orbital cavity. It was true this was not often followed by serious results if the periosteum were intact. Dr. Mosher's modification of the intranasal method of first perforating the infundibulum and upper anterior ethmoidal cells high up above the region of the *agger nasi* instead of the usual method of first entering the bulla (Killian) certainly seemed to possess the theoretical advantages he claimed for it on anatomical grounds, and he (Dr. Hill) was glad to hear that Dr. Mosher had found no great difficulties in successfully putting it into practice. Their object, no doubt, should be to open up as many as possible infected cells which were accessible, but when Dr. Ballenger and others used the expression, "complete ethmoidal exenteration," they were certainly claiming a little too much for operations performed through the nose; for the upper ethmoid cells in some instances formed a quite inaccessible labyrinth in the base of the skull, exceptionally permeating the wings of the sphenoid. He had seen a specimen where the most outlying cell was quite 2 in. distant from the nasal cavity. This was the reason why even a very extensive exenteration often resulted in relief rather than cure, and it was rather surprising that their necessarily

incomplete operations afforded so much relief as they usually did.

Professor Hajek, in his reply, briefly stated that, of course, opinions varied, but he adhered firmly to the principle of seeing everything. He advocated no definite method of operation, for most ethmoids differed; but he believed in removing middle turbinate, and then diagnosed as to extent of trouble, and then the extent of his operative procedure depended upon the extent of the disease. Dr. Ballenger did not desire to defend his technique, but his experience had taught him that the better the drainage the less the danger of secondary trouble.

Memoranda:

MEDICAL, SURGICAL, OBSTETRICAL.

INGUINAL HERNIA IN A CHILD, WITH A SUPRARENAL REST IN THE SPERMATIC CORD.

J. R. H., aged 19 months, was admitted under Mr. Hawthorn on August 14th, 1912, with a large left scrotal hernia, which had been present since one month after birth. A truss had been worn for six months, but without benefit. The patient's general health was good, and both testes were fully descended. The usual oblique inguinal incision was made and the external ring exposed. The external oblique aponeurosis was not divided. The hernial sac, which proved to be of the funicular type, was defined, opened, separated and removed after ligature of its neck. The peritoneum appeared to be quite normal, and no fluid was present. A small oval body, of the size of a split pea and of a yellowish colour, was seen lying on the structures of the spermatic cord. It was remarked that it resembled the adrenal body of the rabbit, and its possible suprarenal nature was suggested. A further suggestion was made that it might be a tuberculous deposit, but against this was the fact that it was unconnected with the peritoneal sac, and also that the peritoneum was apparently healthy, no evidence of tuberculosis being present. It was sent for micro-section. The subcutaneous tissue with Scarpa's fascia was sutured with catgut and the skin united by subcuticular silkworm gut.

The microscopic section of the specimen showed it to be a suprarenal rest (Professor McDonald).

I am indebted to Mr. Hawthorn for permission to publish the case.

Charles F. M. Saint, M.D., M.S.,
Assistant Surgeon, Fleming Hospital for Sick Children, Newcastle-on-Tyne.

DERMATITIS VENENATA.

I have occasionally seen, in summer, dermatitis venenata attacking the hands and arms of gardeners who had handled the cut stems of *Heracleum giganteum*, a plant largely used in the arrangement of perennial borders. In the worst case this year the patient had cut down about two hundred plants, and the hands and arms had become smeared with the sap. Wherever it touched it produced a glow, which was soon followed by intense itching and burning; then an erythemato-vesicular eruption appeared with well-marked infiltration of the skin. Unlike the irritation caused by *Rhus toxicodendron*, it never spread to other parts of the body, but was limited to the areas actually touched by the sap. The toxic agent is probably angelic acid. In its acrid character and sweet aromatic fragrance the sap of Heracleum resembles that of Angelica, the two plants having close botanical relations. Angelic acid, like crotonic acid, is a monatomic acid of the acrylic series. I do not know that Angelica possesses any real antiseptic virtue, but it has always enjoyed a great reputation as a prophylactic against infection—indeed, it is supposed to have derived its name "ab immensa contra venena facultate." Angelica water was in great repute in the Middle Ages as a remedy against the plague, and Fuchs calls it "das aller-edelst wasser das man haben mag für die Pestilenz," and so also du Bartas:

> Contagious aire ingendring Pestilence
> Infects not those that in their mouths have ta'en
> Angelica, that happy counterbane.

As an antiseptic dressing to wounds it was one of the principal ingredients in the balsam of the Chevalier de Saint-Victor which claimed to heal in eight days rapier wounds received in affairs of honour, "à moins que la plaie ne fût mortelle"!

The acridity of the sap of Heracleum, Angelica, and similar plants varies with the season of the year, and all skins are not equally susceptible to its action. A lotion or thick pigment of calamine and creta preparata applied with a soft brush and covered with absorbent gauze gives great relief in this form of dermatitis.

Ficus Carica.—It is not so well known that a dermatitis may be produced in many people by the sap of the common fig. If the acrid milky sap of the cut shoot or leaf-stalk be allowed to drop on the hand it gives rise to a burning sensation with tumefaction at the spot; this soon dies away, but is followed by permanent pigmentation of the skin. I saw recently a patient with ten or twelve round bronze-coloured spots on the back of each hand which were caused by drops of fig-sap thirty years ago. The maculae are not raised, and there is no hyperkeratosis such as chestnut-sap produces. If the hands are washed at once no pigmentation occurs.

In an old manuscript in the Communal Library at Cambrai a number of *recettes* are given which were in use in the thirteenth century. Curiously enough, black stains on the teeth were removed by using a powder of charred fig boughs, so that by removing stains from the teeth the fig may claim to make up in some measure for occasional pigmentation of the skin. In the south of Europe the sap of the fig is in use as a common counter-irritant or vesicant, but it occasionally gives rise to ulceration—"adeo ut liquor ejus, et foliorum succus, non solum mordeat aut extergat vehementer, sed etiam exulcerat et ora venarum aponet et formicarias verrucas exigat."

The fruit of the fig speedily loses its acrid character, becoming sweet and demulcent, and from the days of Hezekiah it has been a popular poultice for boils and sores.

Wilton, Salisbury. Charles R. Straton.

STRANGULATED INGUINAL HERNIA IN AN INFANT.

The report of Dr. G. E. E. Nichols's case of strangulated inguinal hernia in an infant induces me to record the following very similar case.

On July 11th, 1912, at 3 p.m., I was called to see an infant born on May 19th, and therefore seven weeks and four days old. The mother told me that the child, who was breast-fed and previously perfectly healthy, started to scream in the morning, and continued to do so until I saw him. I found a lump filling the right side of the scrotum, evidently an inguinal hernia. The child had vomited two or three times, and looked pale and pinched. Under an anaesthetic it was impossible to reduce the lump, so I removed the child to a nursing home and cut down on the hernia, which was of the congenital variety; on opening the sac I found a coil of small intestine, which could not be reduced until the constriction was divided. The cord was separated from the sac and the latter ligatured high up. The sac contained no fluid. The child made an excellent recovery. I had circumcised the child when a month old.

Howard Dent, M.B., F.R.C.S.,
Wolverhampton. L.R.C.P.

The case recorded by Dr. G. E. E. Nicholls in the Journal of September 21st, 1912, belongs to a class that few have experienced, and is quite worthy of record. This is a good opportunity for stating what has been found out as to the degree of rarity, in early infancy, of strangulated inguinal hernia; and I do this the more readily as I have not long been acquainted with what others have done in this respect.

Those who are not familiar with the very comprehensive and thorough *Treatise on Ruptures*, by Mr. Jonathan Macready (1893), will find it a mine of interest and information on almost everything else relating to hernia, but not on this special topic, which from many other works on hernia is also omitted.

On turning to the *Traité de chirurgie* of Duplay et Reclus, however, the matter is referred to quite clearly

(second edition, vol. vi, page 80) in the article on hernia by Paul Berger (1898):

Strangulation is not found with the same frequency at all ages; it is very rare in early infancy. Gosselin never found it. Holmes, at the Children's Hospital, London, never had to operate in such cases; and the same applies to de Saint Germain at the hospital in the Rue de Sèvres; in ten years Guéniot only operated once at the Hospice des Enfants Assistés. But this accident may nevertheless occur. Heyfelder operated for hernia on a child 8 days old; Dieffenbach, 14 days; Fergusson, 17 days; Dupuytren, 20 days.

Some of this information Berger got from the admirable article by Mr. Howard Marsh in *St. Bartholomew's Hospital Reports* of 1874, vol. x, pp. 205–17, in which 47 cases are tabulated at ages 8 days to 13 years. Of these 16 were under 3 months; 25, 6 months; 31, a year; and so forth. Berger refers to 56 cases collected by Féré, and to several other papers and theses on the same subject, which by 1894 had acquired quite a bibliography of its own.

In Mr. Marsh's memoir, and many of the writings of the time, temporizing measures in strangulated hernia were discussed, but surgeons have long come to see that early operation is not only safer than delay, even in children, but often even safer than previous attempts at taxis, which have their own well-known dangers.

The sole instance I have met with myself occurred twenty-eight years ago in association with Dr. Richard Humphreys and the late Drs. Cayzer and Packer, in private practice in Liverpool. The boy was 28 days old, and the hernia, right inguinal and congenital, had been strangulated two days. I performed herniotomy with the assistance of those gentlemen on June 20th, 1884. The bowel, imprisoned around the testicle in the open tunica vaginalis, was purple, and was reduced without difficulty on nicking the neck of the sac; but the delicate membrane of the latter was not easy to strip up to its offset from the peritoneum in order to tie it with catgut in the inguinal canal, owing to the diminutive size of the parts, which were very little larger than those of a tomcat.

To avoid contamination with urine, the wound was closely sewn without drainage, and a dressing of folded sublimate gauze with a hole for the penis was applied to the front of the pelvis and perineum, and bandaged on with a double spica, in the manner seen by me in Lister's wards in 1881, and described in Watson Cheyne's *Antiseptic Surgery* (1882). Two or three days later the scrotum was punctured with an exploring trocar and cannula, and retained bloody fluid withdrawn, without further need of similar interference. On one of those early days the infant was found collapsed and apparently dying, but revived and made a good recovery, with a sound radical cure and the testicle in proper place in the scrotum. He eventually grew up and entered Sandhurst College and the army.

RUSHTON PARKER, M.B., B.S., F.R.C.S.,
Professor of Surgery in the University of Liverpool.

CAN THE UNDELIVERED FETUS CRY?

THE following cases may be of interest in connexion with the note entitled "Can the Undelivered Fetus Cry?" (p. 653):

1. *Vagitus Uterinus.*—Mrs. C., secundipara, was found on arrival to be in labour; the os was fully dilated, bag of membranes protruded almost to the vulva; the head had not engaged. A hand was thrust through the membranes and, seizing a leg, internal version was performed. During this manipulation the child cried *in utero*, much to the alarm of the mother and nurse in attendance.

2. *Vagitus Vaginitus.*—Mrs. K., also secundipara, was in labour all day, and uterine inertia having set in the midwife sent for help. On examination the head was found to lie in the vagina in an occipito-posterior position. During the application of the forceps the child audibly cried in the vagina.

These are the only two examples met with in an experience of upwards of 3,000 midwifery cases.

London, E. J. REIDY, M.B.

In the JOURNAL of September 14th you raise the question "Can the undelivered fetus cry?" and express doubts as to the possibility. Some years ago, while in Australia, it was agreed at a consultation on a patient of mine, then in her eighth month of pregnancy, that I should at once induce labour. I selected a female catheter as my rubber bougies were, from the effects of the heat, too pliable. I inserted it well up between the wall of the uterus and membranes, when, by mishap, the point penetrated the membranes and the liquor amnii began to flow down the catheter. I was washing my hands in the room, when the fetus gave two or three cries nearly as loud as if it were born. The mother and nurse remarked upon this incident. I was considerably taken aback by it, and began to consider the matter, somewhat timid of the consequences. It occurred to me that after the liquor had drained away air had rushed up the catheter into the uterus, enabling the fetus to breathe. I withdrew the catheter, and in a few hours labour set in, and the child was born without any further interference. Both mother and child did well.

Worthing. A. TEEVAN, L.R.C.P.Edin., M.R.C.S.Eng.

In the Epitome of Current Medical Literature in the BRITISH MEDICAL JOURNAL for July 20th, 1912, you mention a case of this rare condition which had been published by Allard.

I had never believed in the possibility of such an occurrence until some three years ago. At that time I was in practice in Luton, Bedfordshire. One day my partner, Dr. Robert D. Bell, informed me that during the previous night, whilst attending a woman in labour, he had distinctly heard intrauterine crying of the baby, much to his astonishment and to that of two or three women who were present. Had my informant been other than Dr. Bell I should still, to put it mildly, have been sceptical.

A few days later I was attending a lady in confinement. So far as I remember the presentation was occipito-posterior, and I applied forceps. Almost immediately afterwards the child cried distinctly, and the cry was repeated a few minutes afterwards. It was heard by both mother and nurse. Like Allard, I am convinced that Velpeau himself would have changed his opinion had he been present at my case.

Mount Morgan, Queensland. OLIVER SMITHSON.

Reports

ON

MEDICAL AND SURGICAL PRACTICE IN THE HOSPITALS AND ASYLUMS OF THE BRITISH EMPIRE.

THE COUNTY HOSPITAL, YORK.

STRANGULATED FEMORAL HERNIA (GANGRENOUS) WITH FEW SYMPTOMS: OPERATION: RESECTION.

(By R. CHARLES, F.R.C.S., Senior House-Surgeon.)

TIME and again cases of strangulated omental or Richter's hernia have been recorded in this and other journals demonstrating the comparatively few symptoms compatible with strangulation. In the *Lancet* of October, 1911, my brother (Dr. S. F. A. Charles) reported a similar case. In the following case not only was there an absence of vomiting, but the patient had no pain.

C. B., a farm labourer aged 57, was seen by me in the casualty department of this hospital complaining of a swelling in the upper and inner side of the thigh, which he had noticed two days previously. His temperature was normal, pulse 80, and respirations 20; in the region of the saphenous opening was a hard rounded tumour; it was somewhat tender, and the skin over it was dusky and oedematous; there was no impulse on coughing.

My first thought was an abscess, but on reflection and after careful examination came to the conclusion that it was a femoral hernia. Owing to the presence of oedema over the swelling I suspected strangulation with gangrene and advised immediate operation.

On cutting down one hour later a quantity of blood-stained fluid escaped on opening the sac, and a gangrenous entero-epiplocele presented. I opened into the abdomen, packed off the peritoneal cavity, relieved the strangulation

by nicking Gimbernat's ligament in several places, and from within reduced the hernia. I had to be extremely careful, as the gangrenous portion was very soft and lacerable. Resection was performed with end-to-end union and from 8 to 10 in. of the gut removed; subsequently I performed radical cure for the hernia.

The characteristic feature of the case was the complete absence of vomiting and pain, in spite of the fact that there was complete involvement of the gut with absolute constipation; indeed, the patient felt so well previous to the operation that he asked to be allowed to smoke. He stood the operation (which lasted an hour less a few minutes) and made an uneventful recovery, leaving the hospital at the end of three weeks feeling quite well.

It may be of interest, owing to the articles that have been published in this Journal on iodine as the sole preparation and dressing for operation wounds, to state that since writing this article I have carried out this procedure in three cases of acute intestinal obstruction, due to strangulated inguinal hernia with gangrenous intestine. In each case I performed extensive resection—all recovered; also in several other minor cases with most pleasing results. Certainly Mr. Alcock's method deserves a fair trial.

Reports of Societies.

MEDICAL SOCIETY OF LONDON.

Monday, October 14th, 1912.

Sir W. Watson Cheyne, Bart., C.B., F.R.S., President, in the Chair.

The President, in an opening address, referred to the serious loss the society had sustained during the last year by the deaths of several of its most distinguished members. In speaking of the late Lord Lister, he emphasized the truth that not only was Lord Lister the great introducer of antiseptic and aseptic surgery, but also decades ahead of the surgeons of his time in operative surgery—a part of his work which has never been published, and has not been properly recognized.

Anaemia of the Pernicious Type with Acholuric Jaundice.

Dr. F. J. Poynton and Mr. H. C. G. Pedler read a paper on a case of this description. The acholuric jaundice was accompanied by some enlargement of the liver and spleen, great weakness, protracted fever, dilated heart, occasional delirium, and vomiting. The blood count showed at first an increase in the leucocytes, their number reaching 35,200, diminution in the red cells to 980,000, with an increase in their average size and much change in their shape. The fragility of the red cells was less than normal. Temporary considerable improvement had been followed by a relapse which threatened to be fatal. This was accompanied by leucopenia, with the appearance of numerous erythroblasts. Neither arsenic nor neo-salvarsan had influenced the condition. They pointed out such features of resembance to acholuric jaundice as acholuria, enlargement of the liver and spleen, high colour index, and occasional severe anaemia with outbursts of fever. They contrasted the increased fragility of the red cells *in vitro* in some cases of acholuric jaundice with the increased resistance in the case narrated, and also the widely different outlook in the two groups.

Dr. C. R. Box referred to two similar cases under his care. The first, one of megaloblastic anaemia with leucopenia and cholaemia, had been treated with salvarsan on two occasions. Each injection had accentuated the jaundice, but the blood picture had afterwards improved. In the second the cholaemia was accompanied by severe abdominal crises. Mr. Corner had excised the spleen, and the girl appeared to be on the way to convalescence. Dr. Parkes Weber was strongly inclined to think that the case recorded was due to chronic septic infection allied to "endocarditis maligna tarda." Dr. Frederick Taylor mentioned a case under his care of pernicious anaemia and acholuric jaundice in which the red blood count had fallen so low as between 600,000 and 700,000 per c.mm. There were leucopenia and splenic enlargement. Salvarsan had been administered three times, he thought, on the whole, with benefit. Dr. G. R. Ward spoke of two Australian cases, for which excision of the spleen had been done.

Professor Theodore Kocher.

The following letter has been received by the Medical Society from Professor Kocher, in reply to an address of congratulation from the society on the occasion of the celebration of his forty years' professorship at the University of Berne:

To Dr. Mitchell Bruce, President of the London Medical Society, and Members.

Dear Mr. President and Gentlemen,

It was a pleasant surprise to receive the address handed me by your distinguished representative on the occasion of the jubilee of my forty years' professorship, and it will always be to me a most valued possession.

Your Society was one of the first to recognize the importance of my publication on the thyroid gland, and one of the first to confer upon me the Honorary Fellowship at a time when I had little reason to be well known; and it is for this I owe you my warmest thanks.

May your Society, whose efforts are directed in such an eminent and influential manner, continue to flourish to the advancement of medical knowledge, and to raising the standard of the medical profession.

Please accept, Mr. President, officers, and council of the London Medical Society my most cordial thanks, and assuring you of my respect and esteem.

Yours truly,
(Signed) Th. Kocher.

P.S.—To Professor Sir Victor Horsley, who came so far to bring me the address and to greet me on my anniversary, I owe my personal thanks.

ROYAL SOCIETY OF MEDICINE.

Pathological Section.

Tuesday, October 15th, 1912.

Professor R. T. Hewlett, President, in the Chair.

The Structure of Urate Calculi.

Mr. S. G. Shattock remarked that the microscopic structure of urate calculi presented much less variety than those of uric acid. The study was carried out by means of thin sections made after the petrological method. The nucleus, he found, invariably consisted of conglomerated spherules, doubtless held together by vesical mucus. Upon such an aggregate the "body" was formed by a regular *growth* of further spherules, as distinguished from the deposition of such as are independently formed in the urine. That these *grew* upon the surface was shown by the fact that they were hemispheres or lesser sectors only—that is, the centre of the sphere was located on what was at the time the exterior of the calculus, its subsequent growth taking place only on the distal side; the varying proximity of those centres determined the size of the sector produced in connexion with them. From the coalescence of such laterally apposed sectors a continuous lamina resulted. The spheres were resolvable into radiating collections of minute rod like crystals, which the author had called bacilliform. In addition to this method, a laminar formation might arise directly from the radial deposition of bacilliform crystals, radial, that is, to the centre of the calculus as a whole, as distinguished from the centres of individual spheres. The general uniformity of structure was invariably interrupted by the zonular deposition of irregular aggregates of spheres like those composing the nucleus, or of bacilliform crystals without order, or of both intermingled. The various elements might be matched in urinary sediments, but bacilliform crystals in a discrete form were very rare as sedimentary urates. The spherules of calcium urate encountered in calculi corresponded with those sometimes met with in urinary sediments, and were of comparatively large size and constructed of coarse crystals with sharp projecting ends. All urinary calculi contained a delicate organic matrix which was revealed when the salts were dissolved away. The author had shown the presence of this even in an Egyptian calculus of uric acid at least 7000 years old, as well as in intestinal calculi. In 1904 he had independently demonstrated the same matrix in uric acid sediment, an observation previously made by Moritz. That the material in question was mucin was shown by its presence in the urate spheres deposited in normal urine after this has undergone ammoniacal fermentation, and where no protein other than mucin was present. In the

case of calculi, or of sediment, if albumen was reaching the urine from an inflamed mucosa, or through the kidney, albumen might be admixed with the mucin.

Thyroid Action and Reaction.

Mr. Rupert Farrant read a paper on this subject, which will be published in full.

CLINICAL SOCIETY OF MANCHESTER.

At a meeting held on October 15th, Dr. J. W. Grange, President, in the chair, Dr. T. K. Dalziel, of Glasgow, described a disease of the intestine which he termed *Chronic interstitial enteritis*. In this condition the walls of the intestine showed chronic inflammatory changes, the bowel lost its normal elasticity and became more or less brittle. No definite organisms had been discovered on bacteriological examination. A portion or the whole of the intestinal tract might be affected, in the latter case with fatal termination. The main characteristic of the clinical picture consisted in attacks of very violent abdominal pain during the intervals of which the patient might appear quite comfortable and happy. Although the disease might simulate intestinal obstruction, the bowels could always be made to act, even though with difficulty. The treatment consisted of laparotomy and excision of the portion of bowel affected. Should this portion be not very extensive recovery would ensue. A disease identical with this condition had been described as occurring in the lower animals, and on bacteriological examination a bacillus resembling the tubercle bacillus, but shorter, had been discovered.

SOCIETY OF MEDICAL OFFICERS OF HEALTH.

Professor E. W. Hope, in his presidential address on October 11th, said that the phenomenal growth and results of sanitary administration in this country during the last twenty years deserved a very much closer public attention than they had received. The present time was undoubtedly one of great expansion in every field of sanitary investigation. The growing enlightenment of public opinion had enabled sanitary authorities to increase the facilities for investigation as well as to strengthen and perfect the actual administrative procedure with very material advantage to the public. For example, the channels of infection and the conditions which favoured the development of typhoid fever were well known, but the close verification of the actual source of each infection now adopted could not fail to emphasize the value of preventive medicine, and encourage perseverance in them. Another illustration was afforded in the prolonged immunity of the country from small-pox, notwithstanding the growing neglect of vaccination, and this immunity was largely due to improved administrative methods which embraced not only the prompt notification and isolation of the patient and vaccination of contacts, but the greater vigilance and the extended measures adopted at the port of entry. The last dozen years had seen the addition to the statute book of a long list of exceedingly important enactments in matters so diverse as the Midwives Act, the Notification of Births Act, various Acts to protect and further improve the condition of children, Factory and Workshops Acts, amendments of the Public Health Acts, other Acts relating to the improvement of food and milk supplies, the importation of foreign meats, to housing, and, lastly, the National Insurance Act. Numerous orders of great importance had been issued, and, in addition to all these, there had been a considerable amount of special legislation affecting particular districts. One of the means by which the work of sanitary administration might be made more easy and more effective had in recent years been more fully recognized by Parliament in consenting to the enlargement of sanitary areas of administration. If taken in time the growing fringes of cities could easily and cheaply be dealt with upon lines indicated in the most liberal town planning schemes, provided there was unity of administration between the city and its growing suburbs, without which the old bad conditions repeated themselves. The Board of Education and the Local Government Board realized that the improvement in health and physique of children was closely associated with general public health administration, and wisely advised that the work of medical inspection of school children should be carried out in close association with medical officers of health. The obligation of entry into schools which had been placed upon medical officers of health had made it possible to keep closer supervision upon infectious disease as it affected school children and to devise more stringent and uniform methods of control. Whilst open-air teaching might be considered the need of all children, it was absolutely essential for the debilitated and tuberculous, and open-air schools had more than justified the hopes of their founders.

UNITED SERVICES MEDICAL SOCIETY.

At the opening meeting on October 9th, Fleet Surgeon P. W. Bassett-Smith, C.B., in a presidential address, gave some account of the evolution of the Naval Medical Service. From the time of Henry VIII it had progressed in efficiency and in importance. In the reign of Charles I the naval surgeon apparently laboured under the disadvantage of not being supplied with either stores or drugs; but under the Commonwealth a great improvement was manifest, even hospital ships being used by the navy at this time, since when steady preparation for all emergencies likely to occur in either peace or war had been going on, the advance having been especially rapid in recent years. Captain Sylvester-Bradley, R.A.M.C., in a paper on the *Evacuation of the sick and wounded in the Territorial Force*, pointed out that while the field ambulances of the Territorial Force were very efficient, there was little or no organization for medical services on the lines of communication. The general hospital was the only line of communication unit existing in the Territorial Force, and peace establishment of the unit, so far as concerned rank and file, was 66 short of its establishment for war. The clearing hospital being the pivot on which the evacuation of sick and wounded would turn on active service, it appeared urgently necessary that this unit should be organized, and, as far as possible, trained in times of peace. The commanding officer should preferably be a retired R.A.M.C. officer, and should be given ample facilities for seeing and training his unit before mobilization. Rank would seem to be necessary for the personnel, as it would be no easy task to maintain discipline in clearing hospitals full of sick and hungry soldiers. For this reason it would be well to embody the rank and file as reservists of the Royal Army Medical Corps Territorial Force. Transport, such as motor cars and motor buses, should also be registered in peace time, and use should be made of canals and rivers for the carrying of sick and wounded.

LEEDS AND WEST RIDING MEDICO-CHIRURGICAL SOCIETY.

The first meeting of the society for the current session was held on October 18th, the proceedings being commenced by an address from the President (Dr. Joseph Dobson), entitled *Gleanings from general practice*. In its course Dr. Dobson expressed a belief that there was too much readiness nowadays to ignore the wealth of therapeutic knowledge stored up by previous generations of men of great clinical experience. Few medical men who had been in practice for many years could be altogether devoid of great faith in the value of drugs when judiciously administered as a means of relieving the symptoms and discomforts of illness. There was also on the whole a tendency to resort to operative measures too readily. It was doubtful whether it was really wise to include so many scientific subjects in the preliminary examinations of medical students; the time spent in acquiring a knowledge of them would be more profitably devoted to gaining experience of actual medical practice in hospital or private. Subsequently there was a demonstration of cases and specimens, and Dr. Churton raised the question of whether a variable patellar clonus should be taken as evidence of neurasthenia or organic disease in a case of suspected malingering.

Reviews.

GONOCOCCAL INFECTIONS.

We thoroughly agree with Majors Pollock and Harrison[1] that gonorrhoea and its consequences have not received the scientific attention they deserve from the profession in this country. For years the disease has been closely studied by Continental writers, who perhaps have better facilities in special clinics. In our country a gonococcal infection has not been regarded as a serious disease which might lead to permanent deformity, cause loss of life, or diminish the birth-rate. Rather the attitude of the profession has been tainted with a moral bias against the sinner who has been supposed to be suffering no more than he deserves. But the medical profession has nothing to do with the patient except as a suffering individual, and there is no relationship between the sin and the treatment of its results. In *Gonococcal Infections* the authors urge that gonorrhoea and its sequels should be dealt with on strictly scientific lines, and their little book will do much to further this aim. They admit that treatment is still far from satisfactory. The newer organic silver compounds—protargol, argyrol, and others—are useful in making a direct attack on the mucous membrane so long as it is only superficially infected. The vaccine treatment provides what the authors in military phraseology call a flank attack with the object of turning the organisms out of hitherto unassailable parts of the body. But it is acknowledged that when the organisms have fixed themselves deeply in the epithelium of a mucous membrane they may defy any attack; that is to say, "we do not yet know how to cure a chronic gonorrhoea."

The two first chapters deal with pathology and describe fully the biology of the gonococcus, its virulence, the susceptibility of different human tissues, the pathological changes produced by the organism, and the laboratory diagnosis of gonococcal infections. There is a tendency towards prolixity in describing the pathological changes produced by the gonococcus, but the fullness and elaboration are not altogether unwarranted. The treatment of gonococcal infections is fully described. We must commend to the profession the details of treatment of gonorrhoea and its complications given in this book. They follow modern scientific lines. The authors' experience of vaccine treatment is that it has no effect in cutting short the acute stages of urethritis, but that excellent results follow its use in gonorrhoeal arthritis. As to the respective therapeutic merits of vaccines prepared from recently isolated and from older cultures, sufficient work has not been done to justify a definite opinion, but the authors agree that, so far, a stock vaccine prepared from a number of different strains is practically as good as one made from the patients' own organisms. The autogenous vaccine may be used for cases which make no progress under treatment with the stock vaccine.

We have sincere pleasure in bringing this little book to the notice of the profession, as it claims to be a concise account of the present state of knowledge of this important subject. As it is one of the Oxford Medical Publications, it leaves from the bibliographical point of view nothing to be desired.

PRIMARY CANCER OF THE LUNG.

Carcinoma of the lung has always been regarded as a rarity, and many of the leading textbooks pass it over with scant notice. A monograph on the subject written by Sir Risdon Bennett many years ago appears to be the only book specially devoted to it, although many isolated reports are scattered through transactions of societies and hospital records. A larger and more comprehensive work[2] written by Dr. Adler of New York deals with the whole question very fully, and presents a long series of tables in which the details of all the cases are summarized under headings. Many hundreds of such cases are quoted, and although the reports are of necessity somewhat meagre, being culled from many sources, still the whole record has its value from the statistical point of view, and affords material for further study and thought. Dr. Adler glances at the history of the disease, and gives the credit of the first classification of primary growths to Stokes, Graves, Walshe, Hughes, and other physicians of their time, but as to the mode of formation of such tumours there has been endless inquiry and discussion ever since. Etiological considerations show that the civilized races are more prone to cancer of the lung than others, but heredity, contrary to the popular idea, probably has very little influence. Age, on the other hand, has a marked influence, by far the larger number of cases falling in the decade between 50 and 60. The precancerous stages of cancerous affections appear to be as elusive as ever, and although concomitant conditions are well known, the definite relation which they may bear to the disease have yet to be discovered. Contrary to the views of many English pathologists, Dr. Adler does not accept the dislocation of germinal cells and their distribution by way of the lymphatics with any confidence. He regards the epithelial cell as liable to metaplasia under certain conditions, and presupposes some local cause for such change of type. Pathological considerations are thus open to discussion, and anything like certainty has never been attained. The clinical aspect of the disease presents more positive evidence, and diagnosis ought not to be regarded as impossible. Although no one symptom can be regarded as pathognomonic, Dr. Adler maintains that the frequent appearance in the sputa of large granulated, spherical cells, associated with deformed epithelial cells, may be taken as a certain indication of the presence of cancer. His long series of cases includes many that are not primary, and several sarcomatous and ill-defined forms of morbid change. He is optimistic as to the future, and holds out hopes of more certain diagnosis and more successful treatment by surgical interference in early stages.

DIGESTION AND METABOLISM.

In the preface to his book on *Digestion and Metabolism*,[3] Professor Alonzo E. Taylor, of the University of Pennsylvania, states that he is "convinced of the need among American physicians for a work presenting the subject in a popular manner without technical details, and from the standpoint of dynamics rather than from that of analytical statics usually occupied by textbooks of physiological chemistry." He goes on to say that the aim of the book is to "describe the chemical changes in normal and abnormal digestion, and explain the known metabolic modifications that food materials undergo within the body; in a word, his aim is to give the student and practitioner a working knowledge of just what is known to occur in the chemistry of the normal body, and also of the changes concerned in many widespread and important diseases." Although this may have been the author's aim, we doubt whether he can be regarded as having accomplished it, for we have found the book exceedingly difficult to read; it is written in a style which to our comprehension is neither easy nor clear, and it starts from a standpoint which is not at all that of the medical practitioner, but of the advanced student of physiological chemistry with laboratory experience. No doubt it is a compendium of information, and has been brought up to date, but it compares unfavourably with such other recent books on the same subject as the translation of von Noorden's *Metabolism and Practical Medicine*, Starling's *Recent Advances in the Physiology of Digestion*, and Pawlow's work on the *Digestive Glands*, all books that may be read and understood by the practitioner. Dr. Taylor gives few, if any, illustrations drawn from clinical medicine, but overloads his pages with details of laboratory work that lead to no definite conclusion, and will almost certainly be superseded by further discoveries. Is it reasonable to expect the practitioner to attempt to assimilate inconclusive material, and is it not wasting his time to read it unless he is in a position to carry out further researches—a position occupied by few, if any, of those to whom the book is addressed?

[1] *Gonococcal Infections.* By Major C. E. Pollock, R.A.M.C., and Major L. W. Harrison, R.A.M.C. London: Henry Frowde, Hodder and Stoughton. 1912. (Cr. 8vo, pp. 243. Price 5s. net.)

[2] *Primary Malignant Growths of the Lungs and Bronchi: A Pathological and Clinical Study.* By I. Adler, A.M., M.D., Professor Emeritus at the New York Polyclinic, etc. New York and London: Longmans, Green, and Co. 1912. (Med. 8vo, pp. 337; plates 27. Price 16s. net.)

[3] *Digestion and Metabolism, the Physiological and Pathological Chemistry of Nutrition; for Students and Physicians.* By Alonzo E Taylor, M.D. London: J. and A. Churchill. 1912. (Med. 8vo, pp. 560. 18s. net.)

The subject of metabolism is of great and growing importance, and one of the most interesting problems is the metabolism of carbohydrates, which, we are glad to see, is dealt with very thoroughly. Among its obscure problems is the retention of sugar by the kidneys under physiological circumstances. The late Dr. Pavy suggested that the sugar is carried to the tissues in combination with the leucocytes, but Professor Taylor rejects this as unproved, although he has nothing better to put in its place than his belief that the retention is "an expression of semi-permeability resting upon a physico-chemical basis." He lays stress upon the fact that the diabetic organism has lost in part the power of forming fat from sugar, but as we know little about the process by which fat is formed from sugar under normal conditions, he is unable to throw any light upon what happens in disease. Another important question is the formation of oxalic acid, which is also left in darkness.

The chapter on glycosuria is a good summary of our present knowledge, as is also the connected section on carbohydrate metabolism in diabetes, but is the author quite justified in regarding as proved the relation of the islands of Langerhans to the function of glycolysis? He suggests that the non-combustion of glucose in diabetes is due to the inability of the diabetic to split glucose into lactic acid, this power being impaired or lost, while the relative toleration of lactose and laevulose is explained by supposing that the liver has retained its power to convert them into glycogen and to store them. As in acidosis the tissues are deprived not only of soda but of potash and lime, he remarks that our treatment should include all three, and not only sodium bicarbonate; he would give a dose every hour and a larger one at bedtime. Acidosis is discussed further under fat metabolism, and he thinks that most cases may be explained either by the absence of the normal ferment for the combustion of butyric acid or because when the entire maintenance of the heat of the body is thrown upon fat metabolism "there is a quantitative incompleteness in the reaction," as there is more aceto-acetic acid than the body can deal with. Under the metabolism of protein he asks why it is that a dead cell is digested while a living cell resists this action—a consideration worth bearing in mind in connexion with the question so much discussed at the Dundee meeting of the British Association. To say that it is due to the presence of an antiferment is merely a verbal evasion of the difficulty, and until some one can show the chemical difference between a dead cell and a living one it is futile to claim that the problem of life can be solved in the chemical laboratory.

Another interesting section is that on the metabolism of fasting as observed in fasting men and illustrated by the experimental starvation of animals. With respect to the use of large amounts of protein he denies that these are eaten chiefly by the classes that do the most muscular work, even where they can afford to buy them; he thinks the excessive consumption of protein is chiefly among the leisured classes; such excess he regards as wasteful both chemically and calorically, as protein is undesirable as a fuel. He does not even allow that large quantities are desirable for the growing body. With regard to the kind of protein, he thinks it best given mixed; the amount should be about 1 gram per kilo daily for a subject weighing 70 kilos, and for smaller individuals rather more. He thinks plant protein as good as animal protein, but that its value depends upon its bulk, fermentability, digestibility, and variety. Although excess of protein is tolerated in health, this is not true of all persons, and it is never wise to carry an overload; excess in protein is as unwise as excess in tobacco or alcohol, and in no case can be of benefit. The metabolism of old age he believes to differ from that of adult life only in quantity, and this mainly depends upon inactivity and lower input. There is little power of retention of protein corresponding to the reduced intensity of protoplasmic activity, while the sparing power of sugar for protein is very pronounced.

The most effective way to fatten is to give excess of fat if this can be tolerated. The author does not think that the obese are fat because they burn less or have retarded metabolism, but "because they have very effective economy." In many instances of obesity the heat production is not low, but high, and where this is the case there is a very high input. He prints useful tables from Benedict and Joslin on the relation of heat production to metabolism.

The section on fever is short but clear; the author thinks that our defective knowledge is due to the difficulty of studying the question under clinical conditions, but he states that the recent improvements of apparatus and the more accurate definition of caloric efficiency "have placed it in the power of every well-equipped hospital to conduct investigations into the metabolism of patients inflicted with infectious and other febrile diseases." He thinks fever is due, first, to a disintegration of tissue, and, secondly and chiefly, to the action of micro-organisms. There is always an increase of heat production, and where this is not the case the fever must be due to some derangement of heat regulation, as is probably the case, although he does not refer to the matter, in hysterical hyperpyrexia. In addition to the increased production of heat, there is a failure of adequate and regulated heat dissipation.

We have said enough to show that this book deals with many important questions and may be regarded as a valuable work of reference.

MALINGERING.

MR. ARCHIBALD MCKENDRICK'S book on *Malingering and its Detection*[1] is a valuable contribution to a difficult subject, and it may be recommended to those to whom it specially appeals—namely, judges and magistrates, members of the legal profession, and insurance companies. Medical men will also find it helpful in dealing with a class of cases which frequently presents many difficulties. The author points out that the real malingerer is comparatively rare, but that the partial malingerer is more often met with. There is also the unconscious malingerer, or the person who is not malingering with intent to defraud but is acting thus so far as compensation is concerned, attributing his ailment to an injury when it is really due to disease, or he is more frightened than hurt. The last type, being in quest of sympathy, goes from one medical man to another, but, in the author's opinion, eventually finds his way to the electrical department of a hospital. In the chapter on pain, it is wisely pointed out that before a definite diagnosis of malingering is decided, it must be proved conclusively either that there is no existing cause for the symptoms or that the symptoms are at marked variance with the physical signs. In discussing the important matter of stiff joints the value of the *x*-ray examination is indicated; the use of the electrical current for flexing or extending a joint and the use of chloroform are all essential to the settlement of the genuineness of the case. The troublesome question of back injuries is well dealt with, and the section will repay careful study.

In Mr. McKendrick's opinion, radiography is not as fully appreciated as it ought to be. In the chapter on medical examination he points out that since the Workmen's Compensation Act came into force the medical practitioner is called upon to exercise the skill of a lawyer in addition to having a capacity for readily applying his professional knowledge, and that care should be taken to make an exhaustive examination and to consider this with the history of the alleged injury. He gives drafts of a medical report to be used in a case of genuine injury, and also of one to be used in a case of malingering. If medical practitioners would follow these we should probably hear less of disagreement between medical witnesses. We can cordially recommend this book.

THE HOUSING AND TOWN PLANNING ACT.

WHEN the President of the Local Government Board introduced into the House of Commons what is now known as the Housing, Town Planning, etc., Act, 1909, it was found to be a flagrant instance of an attempt at legislation by reference, and so great an outcry was raised that when the bill had been redrafted and reintroduced in the following session it was observed that many clauses had been materially simplified. But even when it passed into law

[1] *Malingering and its Detection, under the Workmen's Compensation and other Acts.* By Archibald McKendrick, F.R.C.S.E., etc., Physician in Charge jointly of the X-Ray, Medical, Electrical, and Balneological Departments, Royal Infirmary, Edinburgh. 1912. Edinburgh: E. and S. Livingstone. (Cr. 8vo, pp. 94. 1s. 6d. net.)

and became an Act of Parliament there were far too many references to previous Statutes, which considerably increased the difficulties of those who had to adminster the Act. In his work on the Act,[5] Mr. W. A. CASSON has endeavoured to lessen those difficulties, by suitable notes on the whole Act, and by introducing in the appropriate place the text of referred sections. One of the most useful provisions of the Act is that embodied in Section 15, which requires that a house under a specified rental shall be let after December 3rd, 1909, the date of the passing of the Act, on the understanding that during the tenancy it is in all respects reasonably fit for human habitation. If such a house can be shown to be not so fit, the tenant can throw up the tenancy and bring an action against the landlord for breach of contract. In this connexion Mr. Casson expresses the opinion that to bring any case within the operation of the section it is essential that there should be a tenancy with a rental—that is to say, that there are excluded from the section cottages let to colliers, or those occupied by agricultural labourers or others, as part of their remuneration, or even cottages occupied by their owners. If this view be correct, it should be noted as one of the matters needing amendment, for there appears to be a very general opinion among housing reformers that the rental of a cottage, especially in rural districts, should be paid direct to the landlord, and should not be included as part payment of wages. The section (18) of the Act which has reference to the demolition of dwellings which have been closed as uninhabitable has been differently interpreted by the Local Government Boards in Scotland and England, and Mr. Casson apparently agrees with the latter authority, for he says that if a closing order has been made and has been in force for three months, the local authority *must* make a demolition order. The English Board has stated that demolition must be carried out even though the owner desired to make use of the premises for a barn, stable, warehouse, etc. In such case the local authority has been advised to refrain from making the formal closing order. The second part of the Act deals with dwellings in the mass as the first part deals with them in detail, and, if properly used, the planning of streets will be greatly facilitated, and the crowding of houses which in the past has been a marked characteristic of many modern towns will no longer be possible. Town planning has no doubt been expedited by the provisions of the Act, but in Mr. Casson's opinion it will probably in course of time be repealed and adopted as the basis of some entirely new legislation dealing with the subject in a more drastic and stringent manner than is the case at present. Since the passing of the Act in 1909 the Local Government Board has issued in connexion with it many Orders, circular letters, etc., and all those published up to September, 1911, are given in an appendix, which numbers seventy-one pages. The value of the work, especially to medical officers of health, is thus very materially increased, for the Local Government Board has not yet seen the necessity for sending these documents to those officials, who have to depend in many instances either upon our columns or the clerks to the district councils for any intimation that they have even been issued.

NOTES ON BOOKS.

As indicated by its title, Dr. SOPP'S book[6] is intended for the public rather than for the profession, although it may be read by any practitioner with advantage, for it gives a perfectly clear and generally accurate account of diabetes, avoiding doubtful questions. There can be no doubt that the co-operation of the patient is essential, but we doubt whether the average English patient would be capable of deriving much benefit even from such a simple and clear account as the author has provided. After all, it seems to require some education and training before people are able to understand medical propositions that are to us perfectly clear; at least, that is the experience of the writer, who has always endeavoured to explain the grounds for his treatment, but has not been very well satisfied with the results. It may be doubted whether improved diagnosis is a sufficient explanation of the enormous increase in diabetes as shown by the mortality statistics of the last thirty or forty years, for although it is probably true that sugar is more often looked for in the urine than it was fifty years ago, those cases of diabetes which terminate fatally have well-marked symptoms which must have attracted attention at any time. With regard to treatment, Dr. Sopp adopts von Noorden's diet and recommends a trial of his oatmeal cure, but he does not appear to appreciate the use of potato where it is desirable to give a limited amount of carbohydrate. For this purpose he gives small quantities of white bread, yet these are so small (50 grams) that they cannot satisfy the patient. Where potatoes are so generally eaten as in England, patients appreciate greatly the amount of this vegetable which can be given as equivalent in carbohydrate contents to white bread, for it is three times as much. The author gives some tables at the end of the book which add greatly to its utility, and we wish such a little manual were available for English practitioners.

Although the general arrangement of the fourth edition of Professor KENWOOD'S treatise on *Public Health Laboratory Work*[7] follows very much that of previous editions, there is evidence in nearly every chapter of very careful revision. For example, in the third edition he suggested that the fact whether a cesspool or drain contaminated a well might be ascertained by introducing aniline red or Prussian blue into the cesspool and endeavouring to detect the colour in the well-water. In the fourth edition he has substituted fluorescine as the colouring matter. On the relative efficiency of a chemical analysis and a bacteriological examination of water, the generally accepted view is expressed that the two processes are complementary to each other, and that it is essential in a great many cases that they should both be performed. Neither analysis is infallible, the value of one is often greatly enhanced by the other, and the value of both depends upon the correct interpretation of results. Separate sections of the work deal with the analysis of sewage and of sewage effluents; soil examination; air analysis; food examination; and the examination of disinfectants. In the section dealing with the bacteriological examination of water, Dr. SAVAGE has introduced a chapter on streptococci, staphylococci, and the meningococcus. The four plates showing objects, variously magnified, found in impure water should be very helpful in microscopic examinations, and the large number of illustrations of apparatus, etc., adds materially to the usefulness of a work which may now be considered as a standard textbook.

In reviewing the diet tables published in Germany by Schall and Heisler the wish was expressed that something similar was available for the use of English-speaking practitioners, so that we welcome this little book on *Food Values*,[8] which has been published by Dr. EDWIN A. LOCKE of the Harvard Medical School. After an introduction which briefly explains the bases of modern dietetics and the method of calculating food values, the book is made up of four tables. Table 1 gives equivalents of weights and measures in the metric system and avoirdupois. Table 2 gives the composition and calorific value of various prepared foods, including meats, fish, soups, vegetables, fruit, cheese, bread, biscuits and cakes, pastry and puddings, condiments, and non-alcoholic beverages. An attempt has been made to make this easier for the practitioner by calculating the value of ordinary servings or slices, the approximate weight of which is given. Other articles of food are measured by tablespoonsful or dessertspoonsful. Table 3 deals with alcoholic beverages, and Table 4 gives the average chemical composition of foods abstracted from the tables of Atwater and Bryant. With the assistance of these tables it is quite easy for any practitioner to compile dietaries containing such definite quantities of protein, fat, and carbohydrate as he may wish to give, and of any calorific value. We heartily commend the book to the notice of our readers.

[5] *The Housing, Town Planning, etc., Act, 1909, with Incorporated Provisions of the Housing Acts, and all Orders issued by the Local Government Board, together with full Explanatory Notes and Index.* By William A. Casson, Barrister-at-Law. Third edition. London: Chas. Knight and Co., Ltd. 1912. (Demy 8vo, pp. 348. 10s. 6d. net.)

[6] *Die Zuckerkrankheit, ihre Ursachen, Wesen und Bekaempfung, Gemeinverstaendlich Dargestellt.* Von Dr. med. A. Sopp. Wurzburg: Curl Kabitzsch. 1912. (Post 8vo, pp. 71. M. 1.50.)

[7] *Public Health Laboratory Work.* By Henry R. Kenwood, M.B., D.P.H., F.C.S., and W. G. Savage, M.D., B.Sc., D.P.H. Fourth edition. *Lewis's Practical Series.* London: H. K. Lewis. 1908. (Demy 8vo, pp. 482; 4 plates and 119 woodcuts. 10s.)

[8] *Food Values. Practical Tables for Use in Private Practice and Public Institutions.* By Edwin A. Locke, A.M., M.D., Instructor in Medicine, Harvard Medical School. New York and London: D. Appleton and Co. 1912. (Post 8vo, pp. 110. 5s. net.)

THE late Mr. Arthur Trehern Norton, C.B., left estate of the gross value of £1,859, with net personalty *nil*.

THE PARLIAMENTARY COMMITTEE ON PROPRIETARY MEDICINES.

THE Select Committee appointed in the early summer to inquire into the sale and advertisement of proprietary medicines resumed its sittings at the House of Commons on October 17th, Sir Henry Norman presiding.

Dr. Norman Walker, Direct Representative for Scotland on the General Medical Council, Physician for Diseases of the Skin at the Royal Infirmary, Edinburgh, and Lecturer on Skin Diseases at the University of Edinburgh, was the first witness. Dr. Walker explained that he attended in place of Sir Thomas Fraser of Edinburgh and Professor Ralph Stockman of Glasgow, who were originally nominated. Professor Fraser was ill, and Professor Stockman had just begun his university session. Dr. Walker said he proposed to confine himself almost entirely to his own special experience in the evidence he would give. The effect upon the skin of unsuitable remedies could be more easily demonstrated to the Committee than their effect upon other organs, for, while internal results could only be surmised, the effects upon the skin could be seen. As it was not convenient to bring patients before the Committee he had brought with him wax casts taken from a number of cases, and they were very close imitations in every respect of the actual appearance of the eruptions, etc. As his evidence proceeded, Dr. Walker produced a number of these models. There were many drugs, Dr. Walker said, which produced eruptions—some important, some unimportant, and some serious. He was not making any accusations in this connexion against proprietary medicines as such; there were many preparations in the *Pharmacopoeia* which might produce harmful effects. His point was that when drugs were administered by a medical man their effects were known and guarded against. Dr. Walker quoted from the card records of his cases, a number of which he had brought with him. One related to a man who was suffering from a rupture, and used an advertised rupture cure which consisted largely of turpentine. He was in the hospital ward three weeks before he recovered from the eruption that resulted from the application of turpentine, and, of course, the rupture remained as before. Some of the drugs administered internally were capable of producing eruptions that resembled certain fevers. An eruption produced by antipyrin was very like that of measles. Opium or morphine externally applied produced a rash so exceedingly like scarlet fever that sometimes it was almost impossible to distinguish between the two. An important branch of the subject was the serious effects produced by the bromides and iodides. Dr. Walker showed a photograph of an eruption produced in a child by the administration of potassium bromide, largely in the form of teething powders. These cases occurred most frequently in districts where married women worked, and where it was important to keep the child at home quiet. Potassium bromide also formed a stand-by remedy for epilepsy, and was an ingredient of many cures for fits. Dr. Walker produced a cast of the leg of a patient who had been taking considerable quantities of bromide. It was originally prescribed by a doctor, but the patient thought he could get it cheaper by purchasing it by the pound, and he took it for years, suffering from this eruption in utter ignorance that the medicine was causing it. To show that his evidence was unprejudiced, Dr. Walker mentioned that after a prolonged search a colleague and he had found a proprietary preparation of bromide which controlled the epilepsy in a particular case without producing an eruption on the skin. Arsenic was exceedingly useful when properly employed, but it was capable of producing effects of a very serious nature. It sometimes produced a thickening of the skin of the palms of the hand and the sole of the foot, which might, and occasionally did, go on to the production of cancer. One of his cases was that of a child of 1½ years who was suffering from an eruption due to receiving ½ grain of calomel three times a day in a soothing powder. It was a very pitiable and common thing to find poor persons starving themselves to provide quite useless remedies for patients suffering from phthisis and cancer. Dr. Walker put forward certain suggestions for which he was glad to find public opinion in Scotland was ripe, the county councils having independently asked for them. These suggestions were that the name and quantity of the active ingredient of a proprietary medicine should be stated on the label; that the label should be a warranty, and that the provisions of the Food and Drugs Act should be applied. Dr. Walker gave it as his own opinion that any proprietary medicine containing a drug that might do harm should have the name of the drug on the label. There would be a difficulty in defining the use of the word "active," and he suggested the appointment of a board that should decide this point. Personally, he did not think the quantity of the ingredients, as suggested by the Scottish county councils, need be stated; it did not convey very much to the lay mind.

In reply to Mr. Lawson, Dr. Walker said that the suggestions of the Scottish county councils were identical with the first three recommendations of the British Medical Association.

Mr. Glyn Jones: Does not your evidence come to this: That no layman ought to take medicines except under the supervision of a doctor?—That is, of course, the ideal.

In reply to Mr. Newton, Dr. Walker said he could, in consultation with medical colleagues, make up a list of drugs which, he was perfectly certain, should not be used in proprietary medicines, and another list of drugs whose suitability was open to argument. He agreed that it would be an advantage if there were a list of diseases which it should be illegal to promise to cure.

Mr. Lawson: Is an additional risk likely to arise under the National Insurance Act, when drugs will be obtained freely by the great masses of the people for supposed diseases?

Dr. Walker: I hope there will be no encouragement to persons to get a prescription and go on using it indefinitely when they have a right to medical advice.

In reply to Dr. Lynch, Dr. Walker expressed the opinion that a declaration on the label of the active ingredients of proprietary preparations would be a step in advance, and would mitigate present evils. The diagnosis of maladies due to wrong use of drugs would be facilitated.

Sir Philip Magnus said that he was inclined to believe that the ignorant public would be more ready to buy a preparation if it was stated on the label to contain ingredients unfamiliar to them than if they recognized the names, and asked the witness if he would approve of a censorship of proprietary medicine advertisements.

Dr. Walker thought that, though theoretically desirable, the result would be an assumption by the public that the remedies advertised were beyond reproach, having passed the censorship.

The Chairman suggested that vendors of proprietary medicines were quite capable of turning a restrictive law with reference to declaration of ingredients to their own advantage. Sir Henry Norman also remarked that according to the witness's suggestion a patient would not have the benefit of the new law until he had come to the hospital and told the doctor what he had been taking. The doctor would then read the label on the preparation and be guided as to what drug was affecting the patient.

Dr. Walker replied that Parliament could scarcely legislate for the very ignorant; if the active ingredients were stated, people of average intelligence would be able to find out something of their effects.

Mr. George S. Paternoster, assistant editor of *Truth*, placed before the Committee some of the information that had come into the possession of that journal during its campaign against quackery. Mr. Paternoster's evidence consisted principally of brief sketches of the careers of various notorious vendors of secret remedies. The list included men with "degrees" conferred by foundations established by themselves, men with veterinary qualifications, and so-called "analysts" who wrote testimonials at a guinea each, the notorious murderer, Crippen, also coming in for mention in connexion with aural institutes. In his evidence generally, Mr. Paternoster dealt mainly with the distribution of nostrums by means of postal advertising. He remarked that the present condition of affairs put a premium upon fraudulent misrepresentation, because an advertiser who was absolutely unscrupulous gained an advantage over one who had some qualms of conscience. The follow-up system, in which a series of letters of gradually increasing intensity were sent to an inquirer,

was the most successful method of proprietary medicine advertising in use to-day. Another method was the wide distribution of free samples followed by a huge advertising campaign. After three months' advertising, the vendor of the preparation expected to get his money back with considerable profits. A plan that had been in continuous operation in this country for the last ten years was to engage lady "health lecturers," who were instructed in a book of "patter" relating to the nostrum offered for sale and in a few elementary hygienic matters. These women held meetings, usually for ladies only, in which the nostrum was recommended. It was remarkable how sufferers from a particular disease became known to the quacks. Lists of sufferers were sold at 5s. or 6s. per 1,000, the charge for lists of consumptives being somewhat higher because this was one of the most profitable classes of patient. Perhaps a man who had sold for years a "consumption cure" would decide to turn his attention to rupture; he sold his list of consumptive patients to an agency and bought a number of "rupture names" to start with.

The witness had not concluded his evidence when the Committee adjourned.

OPENING CEREMONIES AT THE MEDICAL SCHOOLS.

Belfast Medical School.

The opening address was delivered by Dr. Thomas Houston in the Royal Victoria Hospital on October 17th. Dr. Houston welcomed the students; he drew their attention to the enormous changes that had taken place in the last forty years; this change amounted to a revolution and was by no means at an end. But, as ever before, the habits and training students received in their undergraduate career were of priceless importance. The new element or factor lay in the laboratory and in the necessity of highly-skilled and specialized technique. It was by laboratory work that Pasteur forty years ago made his great discoveries and Lister adapted them to surgery. This movement would be embodied in Belfast not merely in the greatly increased laboratory work in the university but also in the King Edward Memorial shortly to be erected in connexion with the Royal Victoria Hospital, in which laboratories for larger pathological, chemical, vaccine, bacteriological, and electrical work would all find place. He then recalled some of his personal experiences as a student, and illustrated the changes that surgery had undergone. It was difficult to see clearly in all such momentous changes, and students should early begin not to accept dogmatic assertion, and to distrust inferences founded on inefficient proof. But although the surgeon had lately been the most useful man in the profession, yet in many cases surgery was a confession of failure. In the future bacteriology must become a study for all medical men and the microscope a most useful, indeed a necessary, instrument.

There were signs of progress; although surgical operations were and would continue to be a necessity in many cases, their sphere was narrowing. In bacteriological infections, often much harm was done by surgical interference. What defence could be brought forward to extenuate a diffuse tuberculosis or tuberculous meningitis following the scraping of a tuberculous gland? Dr. Houston then passed in review the treatments of these infections: following the analogy of the surgical procedure, great hopes were built on the chemical or antiseptic or aseptic treatment; but this failed altogether in internal infections, and was but rarely a success even in skin affections. Syphilis and malaria seemed to furnish exceptions, but they were not strictly bacteriological; however, chemotherapy was a splendid field for future investigators. Bier's method, the Roentgen rays, and others had all their use, but not seldom failed. The expectant method answered in pneumonia and typhoid, but failed utterly in cerebro-spinal fever. Serum-therapy was a magnificent success in diphtheria, and lately in cerebro-spinal fever, where Flexner's serum reduced the deaths in the Belfast epidemic from 80 to 30 per cent.; a potent serum postulated a potentiality in the animal of manufacturing limitless antitoxin to the various poisons which did not exist. Lastly, vaccine-therapy was put on a scientific basis by Wright, who adopted a procedure of Leishman; he began by an endeavour to find a prophylaxis to typhoid. His method and beautiful technique furnished a rational explanation of many phenomena—why massage and exercise agreed in some cases and rest was demanded in others.

Dr. Houston concluded by saying that clinical methods furnished only a presumptive diagnosis; bacteriology, a real diagnosis. Care, infinite care, was necessary; but the profession was rising to a higher level; and it was for each one to see that he or she did the utmost to assist in the elevation.

ROYAL MEDICAL BENEVOLENT FUND.

At the October meeting 19 cases were considered, and grants amounting to £181 made to the applicants. Appended is an abstract of the cases relieved:

1. Widow, aged 53, of M.D.Glasg. Practically unprovided for at husband's death, and children only able to give slight occasional help. Takes boarders but finds great difficulty in making ends meet. Relieved once, £10. Voted £5.
2. M.D.Edin., aged 69. Has practised for several years in a large provincial town, but has been in bad health and at present is incapacited. Voted £12.
3. Daughters, aged 51 and 49, of late M.R.C.S. After father's death helped to support an invalid mother by taking boarders, and have since acted as companions. Income £15 a year each, and one is only just convalescent from a critical illness. Voted £10.
4. Daughter, aged 52, of late M.D. Lost her home a year ago through the death of a brother, and so far has been unable to obtain employment. Voted £10.
5. Daughter, aged 73, of late M.R.C.S. Has supported herself as a housekeeper for many years, but is now past work and has exhausted her small savings. Voted £6 and advised to apply for an old age pension.
6. Widow, aged 45, of M.B., C.M.Glasg. Husband's means were exhausted by a long expensive illness which preceded his death a year ago. Has taken a house in a large town, and hopes to obtain boarders. One daughter, aged 15. Voted £10.
7. Widow, aged 45, of M.R.C.S. Practically unprovided for at husband's death a few years ago, and endeavours to support herself by letting lodgings. Three children, the eldest earning 10s. a week as a clerk, and others dependent. Relieved three times, £35. Voted £12.
8. Widow, aged 72, of M.D. St. Andrews. No income, and family only able to give slight assistance. Relieved five times, £66. Voted £6, subject to application being made for an old age pension.
9. Widow, aged 69, of L.R.C.P.Edin. Income £12 a year, and receives a little help from her children. Relieved once, £10. Voted £10.
10. Widow, aged 46, of M.D. Receives a home at a boarding house in return for her services, but has never been able to obtain a paid post. Only child, a boy aged 14, is at Epsom College. Relieved thirteen times, £146. Voted £5.
11. Widow, aged 33, of M.D.Lond. No income, and occupation practically impossible, owing to feeble health and deafness. Three children, of whom two have been adopted by relations, the third being at school. Relieved once, £5. Voted £5.
12. Daughter, aged 40, of late F.R.C.S. No income, and regular occupation impossible, on account of ill health. Receives 10s. a month from another society. Relieved six times, £72. Voted £12.
13. Daughter, aged 50, of late M.R.C.S. Supports herself by letting lodgings in the summer and keeping a small sweet-shop in the winter, but has had a very bad season, and consequently has no means to buy any stock. Relieved once, £10. Voted £10.
14. Daughter, aged 58, of late M.R.C.S. Had a small income, but lost it through the speculations of a friend to whom it was entrusted for investment, and is now dependent on a relation who can ill afford to help. Relieved three times, £15. Voted £12.
15. Daughter, aged 63, of late M.R.C.S. No income; is in very bad health and consequently dependent on this fund and a small weekly allowance from a friend. Relieved eight times, £90. Voted £12.
16. Widow, aged 72, of M.R.C.S. Is a chronic invalid and dependent on an old age pension and slight irregular help from her children. Relieved seven times, £86. Voted £12.
17. Widow, aged 75, of L.R.C.P.Edin. Supplements an income of a few shillings a week by letting lodgings, but often has her rooms empty and is in failing health. Relieved twice, £24. Voted £12.
18. Daughter, aged 53, of late M.R.C.S. Used to be a nurse, but is now unable to support herself owing to chronic glaucoma. Income £4 a year, and receives slight help from friends. Voted £10.
19. Widow, aged 80, of F.R.C.S. No children and small income insufficient for expenses which are quite unavoidable on account of ill health. Relieved three times, £30. Voted £10.

Contributions may be sent to the Honorary Treasurer, Dr. Samuel West, 15, Wimpole Street, W.

THE "EDINBURGH REVIEW" ON SECRET REMEDIES.

THE current number of the *Edinburgh Review*, issued last week, contains an article by Mr. Hugh Elliot on secret remedies, founded in the main on the two volumes, *Secret Remedies* and *More Secret Remedies*, published by the British Medical Association, and upon a pamphlet issued by the "Owners of Proprietary Articles' Section" of the London Chamber of Commerce. Mr. Elliot makes rather a parade of the impartiality with which he has sought to state the arguments on both sides of the controversy, but his general conclusion is that the evils of the trade in secret remedies can only be checked by the application of the general principle *caveat emptor*, by the spread of education, and by administrative action, apparently by the police, in cases in which charges of obtaining money under false pretences could be sustained. We will not grumble with Mr. Elliot's attempt to be impartial, but must observe that early in his article he introduces, no doubt by inadvertence, a statement which imports some prejudice. "The attack," he writes, "was opened by the BRITISH MEDICAL JOURNAL in 1904." The historical matter of fact is that the series of articles afterwards collected in the volume *Secret Remedies* were in the first place undertaken in order to supply information with regard to the constitution of much advertised remedies to members of the medical profession, who were often questioned by patients with regard to the nature and efficacy of the remedies, and were asked, if not to prescribe them, at any rate to sanction their use. The main object of the publication in a popular form of the materials published, in the first place, for the use of members of the medical profession, was to promote that education which the writer in the *Edinburgh Review* considers so essential.

There is another passage which shows that the judge has not fully understood the case upon which he has set himself to adjudicate. We may make the following quotation the more readily because it is used partly as an argument against the validity of the objection of the trade to the proposal to require the publication on every bottle or package of the name and quantity of each constituent, or at least of every active or dangerous constituent. He says:

"Yet we must remember that proprietary medicines have continued to be very valuable paying properties after their secrecy was divulged. Such, for instance, are antipyrin, urotropin, aspirin, adrenine, and 'tabloid' preparations: for the merits of these substances are recognized so widely that they represent a valuable addition to a doctor's pharmacopoeia."

The inclusion in this list of "tabloids"—practically a trade mark for a method of dispensing drugs of any kind—is evidence that the facts have not been fully understood. Antipyrin, urotropin, and aspirin are registered trade names for bodies of definite chemical composition, which it is open to any chemist to make if he chooses to take the trouble, though he may not apply the trade name to the product. To imply, as we think Mr. Elliot does, that they may be placed in the same class as Beecham's pills, or tuberculozyne, or antineurasthin—to instance three preparations named in the article—is misleading. They are trade names for a particular firm's brand of a synthetic chemical substance of definite known composition.

Mr. Elliot, in opening the case for the proprietors, makes the point that the "attack" on proprietary medicines originated not with the victims of the fraud, nor with philantlropists or social reformers, but with the British Medical Association as representing the medical profession; and he states that it has been suggested that the Association is animated chiefly by the desire to suppress unlicensed competition. The assumption underlying this would seem to be that a man cannot be animated by high ideals and definite aims in any calling in which he makes his living from services rendered to clients. Consideration of the relation between doctor and patient must convince any observer who makes an impartial study of the facts, and has any adequate acquaintance with the history of medicine, that its practitioners, though they demand, like those who follow other professions, adequate reward for their labours, are imbued with certain ideals inculcated by their training, and kept alive by their knowledge of and respect for the traditions of the calling, with the result that when fees are not forthcoming the necessary care and attention will not be withheld from those who need it.

Mr. Elliot fastens upon the statement in the memorandum of evidence put in by the British Medical Association, "that financially the profession gained by the unrestricted use of proprietary medicines and preparations." And we are then presented with the following rhetorical questions: "And if it is really philanthropy which impels the profession along a path contrary to their interests, why, asks the proprietor of secret remedies, does not the Association gather itself together for the greatest achievement open to mankind—the prevention of disease? Why do they not strain every nerve for the extinction of tuberculosis and the other preventable diseases? Why is the interest in prevention among the profession at large chiefly confined to small-pox, where the preventive measures are such as to require every healthy baby born into the world to be delivered into the doctor's hands? Why does the dentist who gaily stops a hole in our teeth not inform us of the measures requisite for preventing the arrival of future holes?" Mr. Elliot admits that the exaggeration, and, indeed, the baselessness, of this criticism will be obvious to every well-informed person, but he might have pointed out that the enormous progress which has taken place in the theory and practice of preventive medicine within less than a century is almost entirely due to the investigations and advocacy of members of the medical profession, who have by their persistent efforts partly converted public opinion. The results of its efforts are reflected in the public health legislation of the last half-century, and in the elaborate system of public health administration which has been called into existence. Such a charge as is implied in the question is refuted also by the modern development of the treatment, preventive and curative, of many diseases by appropriate serums and vaccines, to the notification of tuberculous disease, to the early treatment of ophthalmia neonatorum, and, lastly, to the enormous amount of time and money that is devoted to the study of cancerous disease. To the discovery and application of means for the prevention of disease the medical profession is devoting time, energy, and money. In this good work the British Medical Association has not been backward; indeed, it may justly claim to have taken the lead.

There are several reasons why charges are not more readily preferred by the victims of the nostrum-monger. One is that no man is ever ready publicly to admit that he has committed a foolish act; another is that, the greater number of proprietary remedies being composed of harmless ingredients, it is only in very flagrant cases that the damage becomes apparent to the victim, either owing to delay in obtaining suitable treatment in serious disease, such as cancer or tuberculosis, or by the exhibition of a drug that may be harmful from its inherent qualities or from its prescription without due regard to the nature of the individual case. Self-medication without due regard to the requirements of the individual must always be unsatisfactory and often dangerous. It is not difficult to conceive a case in which the prescription Mr. Elliot gives as an illustration of the safety of self-drugging—"℞ Pil. hydrarg. gr. iiij"—might imperil life.

This brings us to another point upon which the writer in the *Edinburgh Review* shows himself an uncertain guide. He quotes from the pamphlet issued by the London Chamber of Commerce the claim that proprietary medicines constitute a system of medication that is unsurpassed for simplicity, uniformity, and cheapness. The claims as to simplicity and cheapness we may pass by as too absurd to need refutation. But about uniformity there is something to be said. It is abundantly proved that secret remedies are not uniform. In the first place, their composition may be changed, and in certain instances has been changed, without any alteration in the name; and in the second place, many are so imperfectly dispensed that the dose varies within very wide limits. Certain headache powders, for example, have been shown to be very far from uniform in their compounding, the amount varying in different powders or tablets within quite wide limits. As a contrast to this let it be noted that one of the chief purposes of the *British Pharmacopoeia*, the official publication issued under the authority of Parliament,

is to ensure the uniformity of drugs. Any one who will take the trouble to consult that volume will find that minute directions are given to ensure uniform strength and the absence of impurities. The prescriptions of the medical profession are made up according to the direction of the *British Pharmacopoeia*, and uniformity is thus ensured. The technical terms, such as "Pil. Hydrarg.," which excite the easy ridicule of some critics, are safeguards to the public because the use of the technical term implies that this preparation must be that directed by the *British Pharmacopoeia*.

It is true that doctors are not infallible, but it is true also that not all diseases are curable; doubtless cases occur in which medical science is unable to cure, arrest, or sometimes, though the number is yearly decreasing, even to identify the disease, but such a statement is no argument for the safety or the efficacy of proprietary medicines, any more than it is a sound proposition to advance the fact that bed is the most dangerous place in the world, because most people die there. Patent medicines are like ready-made clothes, for although in certain cases they may serve a useful purpose, they are administered without regard of the needs of the individual, and it remains purely a matter of chance whether the fit is good or bad.

A NATIONAL MEMORIAL TO LORD LISTER.

A large and influential meeting was held at the Mansion House, London, on October 23rd, for the purpose of establishing a fund for a memorial to the late Lord Lister. This, it is proposed, should take the form of a medallion to be placed in Westminster Abbey, a monument to be erected in the metropolis, and the establishment of an international Lister memorial fund for the advancement of surgery. The Lord Mayor presided, and the assembly included the Lord Chancellor (Lord Haldane), who attended in the absence, through indisposition, of the Prime Minister; Lord Avebury, Lord Cromer, Sir Archibald Geikie (Royal Society), Sir Rickman Godlee (Royal College of Surgeons), Sir Thomas Barlow (Royal College of Physicians), Sir Francis Champneys (Royal Society of Medicine), Professor Saundby (British Medical Association), Sir Donald MacAlister (General Medical Council), Sir William Osler (University of Oxford), Professor Howard Marsh (University of Cambridge), Sir Alfred Pearce Gould (University of London), Sir John Rose Bradford, F.R.S. (Honorary Secretary to the fund), and many others.

Establishment of a Memorial.

The Lord Mayor said that the purpose of the meeting was to inaugurate a lasting memorial to one of the most eminent men who had adorned the profession to which he (the Lord Mayor) had the honour to belong. The nation gave great prizes and distinctions to those who could invent engines of destruction of life and limb, but it thought too little of the skill, intelligence, and ingenuity of those who had saved life and limb, and had mitigated suffering in thousands of cases. Such an one was Lord Lister, who devoted a life of study to the great discovery with which his name was associated.

The Honorary Secretary (Sir John Rose Bradford) said that letters regretting inability to attend had been received (amongst others) from Prince Alexander of Teck, Mr. A. J. Balfour, the Archbishop of Canterbury, Sir James Whitehead, the Ambassadors of France, Russia, the United States, the Ottoman Empire, the Belgian Minister, and many members of the House of Lords and House of Commons. The Archbishop of Canterbury wrote that it would be difficult to exaggerate the value of Lord Lister's services to humanity and the animal creation generally; and Mr. Balfour felt confident that from the nature of the cause the appeal would meet with ready and substantial support.

The Lord Chancellor (Lord Haldane) moved the first resolution:

> That this meeting is of opinion that the priceless services of the late Lord Lister to the cause of science and the alleviation of human suffering should be commemorated by a suitable memorial.

Lord Haldane remarked that he had just come from the house of the Prime Minister, whom only the stern injunction of his physicians that he must husband his strength for unavoidable duties prevented from being present. Mr. Asquith would have wished, as head of the Government, to move the resolution, and to testify on behalf of the Government to the importance which it attached to this memorial. Lord Haldane mentioned that he had the honour of knowing Lord Lister, and was privileged to be one of the spectators at that extraordinary tribute paid after his death to his memory, not merely by the people of this nation but by the potentates and peoples of other great nations. A distinguished foreign critic once observed that the British were a remarkable people—they were not great at organization, probably the average number of highly educated men was not as great among them as it was in some other countries—but the British race had this remarkable quality—it poured out a succession of extraordinary leaders of men in science and in literature. Every now and then somebody appeared who was outstanding, not merely amongst his fellow countrymen, but in the world. Lord Haldane grouped the names of Darwin, Kelvin, and Lister as three men who stood for much more than pre-eminence among their fellow Britons. Lister in some ways was different from the other two. He did not inaugurate a great world-wide, far-reaching conception such as that which underlay the doctrine of the *Origin of Species*, nor was he associated with matters resembling those far-reaching applications of the theory of mathematics and the principles of physical science which were the outcome of the inventive genius and imagination of Kelvin. Lister did something in its way more extraordinary. He gave to the world a discovery which revolutionized the whole conditions of the practice of surgery. Not only did the terrible diseases of the surgical hospital almost disappear, but the work which Lister did opened up a whole new field, which had since been explored in a fashion that had had the most fruitful results. The basis of his discovery was his treatment of micro-organisms, and his principles set men thinking so that they were speedily transferred to the science of medicine as a whole. It was hardly too much to say that Lister's discoveries had been almost as fruitful in turning men's minds to the study of micro-organisms in medicine as they were in his own department of surgery. In surgery it was not only that a whole series of surgical operations, previously attended with the utmost danger, and resulting in death in a large percentage of cases became capable of being performed with comparative ease and certainty, but a whole field of new surgical operations were rendered possible, which no one but a madman would have attempted before Lister made his discoveries. That was a tremendous revolution, and in time it came to be accepted as such all over the world, so that now a surgeon would as soon think of neglecting Listerian precepts as of going without his food In medicine the attention which had been directed to the effect of life in these minute forms upon the human organism was no less marked; the discoveries that had been made in pathology, in tropical diseases, plague and all other maladies attendant upon the introduction of micro-organisms, were due to the new zeal stimulated by Listerian methods being devoted to the cognate study of medicine and the life of the organism as a living whole. The man who did that effected a revolution perhaps more definite, more precise, more far-reaching than anything done by even Darwin and Kelvin, important as the changes were that they introduced, and this country might be proud of the privilege of seeing arise from its ranks such a man as Lister. Another legitimate source of pride was that Lister's work shared that splendid privilege of scientific accomplishment that it was done not for one nation but for humanity at large; it was as accessible and valuable to every other nation as it was to theirs; in science was to be seen that noble spectacle of the co-operation of nations for great and peaceful purposes for the good of mankind. Lister was one of the great pioneers in that kind of altruism, and it was not unnatural that he should come to be regarded, even in his own lifetime, as a man who belonged not to Great Britain but to the world, and of whom the world was consequently proud. It was fitting that the Government should be represented at such a gathering as this, for there was no part of the community that had not profited by Lister's discoveries; all were his debtors, the humblest and the greatest. The poor had benefited, perhaps, even more than the rich, and therefore it was fitting that the meeting should assume a national aspect, and that in the

name of the whole nation honour should be done to the memory of one of the greatest men of recent years.

Sir Archibald Geikie (President of the Royal Society) seconded the resolution. He described the late Lord Lister as at once one of the greatest men of science the world had ever seen and one of the most signal benefactors of humanity as a result of the work to which he gave his life. Sir Archibald briefly reviewed Lord Lister's career and the discoveries by which he was able practically to banish from operations the malign demons of inflammation and suppuration, and to enable the most difficult of operations, and new operations, to be performed with ease and success. He threw a flood of fresh light on the relations of bacteria to disease, and, in conjunction with Pasteur and Koch, Lister might be regarded as one of the founders of the new science of bacteriology. His name would be handed down to future generations as one of the chief alleviators of human suffering. Into every hospital in the world he had brought hope and confidence, with diminution of pain and death, where before his time despair, prolonged suffering, and mortality were rampant.

The resolution was put to the meeting and carried unanimously.

The Form of the Memorial.

Lord Avebury moved the second resolution:

That this meeting cordially supports the scheme which has been adopted by the General Committee appointed for the purpose of deciding what should be the form of the memorial, namely:

A memorial in Westminster Abbey, to take the form of a tablet with medallion and inscription.

The erection of a monument in a public place in London.

The establishment of an International Lister Memorial Fund for the advancement of surgery, from which either grants in aid of researches bearing on surgery, or awards in recognition of distinguished contributions to surgical science should be made, irrespective of nationality.

Lord Avebury referred to the many directions—politics, music, art, literature—in which this country had deserved well of the world. In medicine the great names of Harvey, Jenner, Simpson, and Lister would never be forgotten. Of Lister it could be said that, with the possible exception of Pasteur, no one had done so much to mitigate the sufferings of poor human nature. Lord Roberts once remarked to him (Lord Avebury) that no one except those familiar with the condition of military hospitals before Lord Lister's time could possibly realize the value of his contribution to medical science.

Sir Rickman Godlee (President of the Royal College of Surgeons), in seconding the resolution, alluded to the heavy responsibility of speaking for the medical profession, and especially the surgical part of it, on this occasion. However, he could claim that very few of those present had had a greater opportunity than himself of seeing the enormous change that had come over the domain of surgery in the last forty years, chiefly through the discoveries and the enlightened perseverance in putting them into practice of the great man to whom it was proposed to raise a memorial. It might almost be said to be fortunate that Lister had charge of what were at the time perhaps the most unhealthy wards in the kingdom, because the reduction in mortality that took place consequent on his reforms was extremely striking. In a short time those wards became more healthy than any others in the world at the same date. If it were possible to draw a picture, what a contrast there would be between the wards of the early Seventies and those of to-day! The nature of the cases susceptible of surgical treatment was now vastly different; surgery had advanced in every direction, and now cured or relieved a great number of diseases which formerly were considered incurable. Hardly any region of the body was now beyond its reach, and conditions which in the old days were only helped by amputation were now relieved by milder means. Lister's labours continued to yield fruit of incalculable value. Let it be remembered that he was not merely the promulgator of one new doctrine, but a practical surgeon of the highest eminence. Sir Rickman Godlee mentioned the various proposals for a memorial to Lord Lister that had been considered by the Executive Committee, and laid emphasis on the international character of the awards to be granted from the memorial fund.

The resolution was carried unanimously.

The Hon. W. F. D. Smith moved a vote of thanks to the Lord Mayor, and referred to the institute with which Lord Lister's name was latterly associated, and also to the personal characteristics of the scientist whom the whole world held in reverence. Lord Lister was honoured by his Sovereign and by his fellow-scientists, but his greatest reward was the consciousness that his discoveries had saved a great sum of human life, and alleviated a vast mass of human suffering.

In acknowledging the vote of thanks, which was carried with acclamation, the Lord Mayor alluded to the reforms in hospital practice which in his experience had followed the establishment of Listerian principles.

The meeting then terminated.

Donations to the fund may be sent to the Treasurers of the Lister Memorial Fund, at the Royal Society, Burlington House, W.

LITERARY NOTES.

The papers, speaking of Mr. Roosevelt, who was so fortunately saved from death by the bulk of the manuscript he carried in his pocket, say his "stamina is remarkable." We read the statement with great pleasure, but we wish it had been expressed in better grammar. As "stamina" is so often treated as a singular form, we hope it will not be thought pedantic if we point out that it is plural. The singular is "stamen." The word recalls the story of Thomas Willis, who, when called in to see one of the sons of the Duke of York, afterwards James II, gave his opinion in the words, *Mala stamina vitae*, which gave such offence that his advice was never asked again by the royal father. "Stamina" as a singular noun is as grotesque a barbarism as "phenomena," which we remembered seeing followed by a singular verb in a letter addressed to a contemporary by one of the most distinguished physicians of the day. The solecism was all the more remarkable, since the writer was somewhat noted for his literary style.

Some three years ago we were gravely taken to task by a correspondent for allowing a contributor to speak "of women who are but fighting for a just cause" as "Suffragettes." We pointed out that the word was frequently used in the editorial as well as the advertising columns of *Votes for Women*. Now we note that the word has received what may be called official consecration, as it is the title of the journal "edited by Christabel Pankhurst, LL.B.," the first number of which appeared on Friday, October 18th. The foreword to that instructive periodical says:

The name "Suffragette," first applied to members of the W.S.P.U. by the newspapers, has, by use and association, been purified of any opprobrium or distasteful significance it may have borne in the past. It is now a name of highest honour, and women in ever-increasing thousands bear it with pride; and until a better is invented it stands as no other word does for the independence, courage, public spirit, and, we may add, humour, which are the attributes of the really womanly woman.

Further, we are told that "the suffragette has come to stay." Whatever high qualities the suffragists may have shown, it certainly had not struck us that humour was one of them. But there is something deliciously humorous in the assurance given, since the lady who may be presumed to have written this finds it, we understand, inconvenient at present to stay in the country in which *The Suffragette* is published.

Referring to an article on cramp rings which appeared in the Journal of October 19th (p. 1072), Dr. Y. M. Jones-Humphreys (Cemmaes, Montgomeryshire) writes:

It may interest some of your readers, and perhaps some of them can give a better account of the superstition than I can, that rings to "cure fits"—that is, probably epileptic—were worn by people in Wales in the eighteenth century, but the following process had to be observed:

1. To collect twelve pence singly from strangers.
2. To exchange the pence with a stranger for a shilling.

The shilling was then made into a ring. I remember one being made on the thin end of a blacksmith's anvil by being beaten with a hammer.

I can also remember a woman collecting pence from strangers while travelling by train; her husband was with her at the time, and had a ring on one of his fingers, from wearing which she said he had derived great benefit. How many rings were necessary to effect a complete cure she did not say.

British Medical Journal.

SATURDAY, OCTOBER 26TH, 1912.

A SCIENTIFIC STUDY OF MEDICAL BENEFIT.

Mr. I. G. Gibbon, D.Sc., who some time ago published a work on unemployment insurance the result of research in the Department of Sociology in the University of London, has now issued another volume on *Medical Benefit*.[1] It is founded on a study of the experience of Germany and Denmark, and, as in his earlier work, Mr. Gibbon approaches the subject in the impartial spirit of the disinterested scientific observer; he has not set out to prove any thesis, but has collated and analysed the facts of observation and set out the conclusions to which in his judgement they lead. Mr. Gibbon's treatment of the subject is absolutely unemotional, and it is interesting to observe that in the main, and with the exception of certain points of detail, his conclusions fully justify and support the contentions advanced by the British Medical Association in the name of the profession in its criticisms of the medical benefit proposals of the National Insurance scheme as originally put forward. Some of these objections have been met by Parliament in consequence of the representations of the profession, and on some others it is clear that the views commonly held by members of Parliament and by the representatives of the working classes fall far short of the ideal which Mr. Gibbon would put before them.

In Germany, where the law has recently been amended so that it more nearly accords with the principle of the British scheme, sickness insurance including medical benefit is compulsory, but in Denmark it is voluntary, although the State exercises a certain amount of control since it makes a contribution to schemes which fulfil certain conditions it lays down. In Germany sickness insurance comprises medical treatment, drugs and appliances, and money benefit, or as an alternative to these three, institutional benefit with, if the member has dependants, reduced money benefit. Maternity benefit and death benefit are also provided. Invalidity insurance comprises a pension for permanent disablement or for temporary disablement after twenty-six weeks, an old age pension after 70, and, at the discretion of the insurance authorities, treatment for an ailment which threatens premature invalidity. A large amount of sanatorium treatment for tuberculosis is given under this head.

We must not set out here all the conclusions to which Mr. Gibbon has been led by his facts, but may indicate some of the most important. The facts, he says, show that medical service should be provided mainly in kind, and not by giving the insured person a sum of money and leaving him to make his own arrangements. Free choice of doctor should be ensured in the interests both of the doctor and of the patient, but he considers it essential that strict control should be exercised over medical practitioners in their treatment of insured persons to prevent such abuses as laxity with regard to malingering, unnecessary visits, and unnecessary ordering of expensive drugs and appliances. On the other hand, the facts, he holds, prove that detailed control in expert matters should be exercised mainly through the organization of the doctors themselves—the difficulties which have arisen in Germany pointing to the conclusion that expert should control expert. With regard to remuneration, he says that the facts indicate that medical practitioners will demand, and will probably obtain, a higher rate of remuneration from the working classes than has been general in the past. Further, while the payment by the insurance authorities for medical services should be in the form of capitation fees, he holds it reasonable that individual doctors should be remunerated according to services rendered, and that for this purpose insurance authorities should make contracts with corporations of doctors, paying the corporation the capitation fees, and leaving the corporation to divide the proceeds among the several doctors according to their respective services.

The evidence he has obtained from Germany leads him to the conclusion that valetudinarianism is a greater menace to the successful working of sickness insurance than deliberate malingering. It is a danger, he says, which hangs on the skirts of all insurance against sickness, or invalidity, or accident, a habit of mind ready to fall victim to disease, and quick to exaggerate small symptoms to gross proportions, and to make a person ill by mere weight of thought. He does not think that the evil in Germany is due solely, perhaps not even primarily, to insurance, which is but one of a number of causes. "In recent years," he says, "men have become more and more alive to the part which mind bears in the causation of disease. Yet, unavoidably, the very efforts to cope with disease may seriously weaken the mental barriers to its inflow. On every hand institutions for dealing with disease are multiplied, insurance schemes among them, and the very fact of doing this emphasizes the existence of disease. Each institution serves as a point of suggestion for ailment. The youth who read the medical treatise thought that he had every ill described except housemaid's knee. The ordinary citizen is becoming surrounded by institution after institution for coping with disease. There is danger that they will do for him what the medical treatise did for the youth; and they are always open before him. The influence of the clever advertising of patent medicines is also very great. The danger is a very serious one, much more serious than deliberate malingering. The poison works more subtly and insidiously. It saps the mental resistance to disease." He concludes that experience proves that for this reason alone there are considerable advantages in making the insured person pay for part of the cost of medical service out of his private resources. It would favour the better development of the system of educating the insured public as to medical treatment and in matters of health, which is a necessary part of any sound health insurance scheme, and the education might perhaps check valetudinarianism.

Experience, he says, proves that institutional benefit is essential for adequate medical treatment, but that such benefit should be restricted within reasonable limits. To achieve this medical practitioners should be remunerated in such a manner that they have not any pecuniary inducement to send cases unnecessarily to hospitals. It does not pay an insurance society, much less the community, that cases which under a judicious system of medical remuneration could

[1] *Medical Benefit: A Study of the Experience of Germany and Denmark.* By I. G. Gibbon, B.A., D.Sc. London: P. S. King and Son. 1912. (Demy 8vo, pp. 311. 6s.)

be equally well treated at home should be sent to hospital, and it is partly on this account that remuneration paid according to the services rendered by the individual doctor, and not by a capitation system, is advantageous to the community. Full advantage of domiciliary treatment cannot, however, be obtained without the provision of an adequate system of home nursing. With regard to the wage limit, the conclusion is that the claims of the doctors that a distinction should be drawn between the terms on which medical service is rendered to persons with comparatively large earnings and persons with small earnings merit sympathetic consideration.

Mr. Gibbon, as we have said, looks upon the problems which so nearly concern the medical profession with the indifferent eye of the impartial scientific observer. It is, therefore, all the more interesting to find him coming to the conclusion that the medical profession should corporately arrange with the insurance authorities for medical service, with indeed a liberal dash of individual stimulus. This conclusion may be commended to the attention of Drs. Harford and Mills, who in their pamphlet have sought to convince the profession that the scheme of a public medical service conducted by the medical profession is impracticable and contrary to public policy. Mr. Gibbon goes further, and asks why, if doctors should corporately undertake medical service, a principle which he regards as the inevitable consequence of the facts he has accumulated, their corporate responsibility should not be extended to hospital as well as domiciliary treatment. The different kinds of treatment, he observes, cannot be divided by watertight compartments; there must be organic relationship between them. He thinks that there is a risk that this will not be secured satisfactorily and permanently unless all are under one jurisdiction, and he believes that if doctors were corporately responsible for hospital treatment with a pecuniary interest in it, the difficulty of checking the abuse of hospitals by doctors—that is to say, of checking the sending of patients there without sufficient reason—would be largely solved. He sees three possible methods by which hospital treatment may be provided for insured persons—namely, by public hospitals, either State or municipal, by hospitals run by insurance societies or authorities, and by hospitals under the direction of the organized medical profession. The experience of State or municipal hospitals gained on the Continent is not such, he says, as to make the impartial observer very enthusiastic of the results. Against the provision of institutional treatment directly by the insurance societies and authorities is the objection that the organic relationship between forms of medical service is lost. He acknowledges that the tendency at present is towards municipal provision of institutional treatment, but believes that the permanent development may lie in quite another direction. He believes that, while the capital expenditure on buildings and equipments may be provided by public authorities or by insurance societies or authorities, the essential matter is that the treatment should be under the primary control of the organized doctors. If, he says, "society is to be organized increasingly, and with stability, specialization of function by groups will grow," and he points out that in Germany in some places the doctors have gone beyond corporate bargaining and undertaken corporate liability for service and control. The workman, Mr. Gibbon thinks, ought to realize that, if he imposes needless control over the work of the doctor, whether individually or through his society, or if he imposes in either of these ways a control which could be equally well exercised by the doctors themselves, he is "violating the demands of democracy no less essentially than does a tyrant who exacts forced labour at the point of the bayonet." Mr. Gibbon says that in the scheme of medical administration suggested by his observations, the conclusion is that the doctors should organize their own system of control subject to the general supervision of those to whom they render service. He contends further that this conclusion deduced from practical needs affords the best form of control, and is seen to be in accord with the broad claims of democratic development. He recognizes that a system under which there is free choice of doctor clearly implies that ordinarily every doctor should be free to join the medical organization, and that each should have an equal voice in the management and control: this is the ideal of the British Medical Association.

THE SECRET OF LONG LIFE.

WHENEVER any more or less distinguished person reaches the age of 80 he is at once subjected to a close interrogatory as to his habits of life, in the hope that the secret of his long life may be discovered. The other day Mr. Frederic Harrison celebrated his 81st birthday, and confided to a representative of the *Daily Mail* "five golden rules of health." They are as follows: "(1) Abstain from tobacco, spirits, made dishes, and all such dreadful things. I am satisfied with a little bit of mutton and rice pudding. (2) Rise from a meal with an appetite. I believe people eat too much. (3) Walk every day for two hours. This I am going to do as soon as I get through a pile of letters and telegrams from Florence and Rome. I am too old to play at tennis, and golf is too slow. (4) Sleep eight hours. People cannot sleep who smoke themselves black in the face, eat too much, and have not walked enough. (5) More important than all—be content with what you have got. Take things quietly." Lord Strathcona, who is 92, expressed approval of these rules, adding details of his own personal habits. He has not smoked for the past seventy years, and does not believe in smoking. He thinks that most people eat too much; for many years he has only taken two meals a day, breakfast and dinner. He eats little or no meat, and the diet agrees with him. Exercise is decidedly, in his opinion, a most important factor of good health and longevity, but, like Mr. Harrison, he has a great deal of correspondence to go through; hence he cannot always find the time for walks. He makes a point of not sleeping longer than six hours a day. Tolstoi, the grand old man of Russia, taught that the secret of long life is to be found in the following hygienic code, which is now, we believe, being circulated among his countrymen: Fresh air, day and night; daily exercise; moderation in eating and drinking; one hot bath weekly, and a cold one daily; comfortable and not over-heavy clothes; a dry, spacious, and sunny dwelling; scrupulous cleanliness; regular work, which acts as a preventive of ills of body and mind. Rest after labour, he teaches, must not be sought in distractions. Night was intended for sleep; the chief condition of good health is a life fruitful in labour and ennobled by good actions. Moltke in his 90th year said he had maintained his health by great moderation in all things, by regular outdoor exercise in all weathers, good and bad, never a whole day at home. In an article by Mr. H. Ottewill Bruce, which appears in

the current number of *Chambers's Journal*, the results of an investigation of twenty-four recent American centenarians are given. Eighteen of them were women, and previous investigations have shown that women have a greater predisposition to longevity than men. This may be due to the fact that as a rule they lead a more protected and less strenuous life. Otherwise the experience of the individuals adds little to what has already been said. It is to be noted, however, that some attributed their length of days to simple living—and no medicine. Simple living—which, by the way, is a very different thing from the highly artificial and often unwholesome existence sometimes known as the simple life—is excellent; but the implication that the avoidance of medicine tends to lengthen life is a confusion between cause and effect. The triple brass of a sound constitution will bear unhurt shocks which make occasional tinkering necessary for a weaker vessel. It is significant, however, that the advice comes from America, where the consumption of quack medicines is very great and widespread. Self-drugging is undoubtedly an evil to be avoided by any one who wishes his days to be long in the land. Then the methods of the vendors of quack remedies, with their circulars full of pseudo-scientific descriptions of disease and its effects, create an unhealthy state of mind. The microbe is too much with us. One lady thought the secret of longevity was to be found in the *dive bouteille*, for her prescription was burgundy four times a day! The most comprehensive replies were given by a negress, who attributed her long life to a good constitution, and a tramp, who said he had never followed any rule of life. All the centenarians in this list, it was noted, were married, and the writer of the article says he has no recollection of ever coming across a record of an unmarried centenarian, man or woman. This is a fallacy which has been exposed several times. It is obvious that more persons of sound health will marry than people of bad constitution. It may be, too, that there is something in the fact that the responsibilities of married life tend to make a man put forth his best endeavour, and saves him from the perils of idleness. It may further be allowed that a married man is, as a rule, better looked after than a bachelor. This, however, scarcely accounts for the large proportion of women among centenarians, unless it be that, contrary to the doctrines of modern feminism, marriage is, after all, woman's natural profession.

The precept not to worry, which we find so often repeated, is sound in itself; it is in the application that it is difficult. If a person has nothing to worry about, there is no fear of the fretting of the mind wearing out the body. It has been cynically said that a bad heart and a good digestion form the best equipment for long life. It has often been noticed in men or women who have led a life of great nervous strain, that when they get into the sere and yellow leaf their mind becomes less active, and the body at once, as if relieved from the constant mental goad, becomes healthy. The absence of wear and tear of mind is probably one reason why most centenarians are found in the country, usually among the poor and ignorant classes. Not a few of them are mere wrecks who have drifted into workhouses. They have not worried because they have taken things as they come with the fatalistic resignation of the poor in spirit. The intellectual and other problems which press so hard on this generation leave them untouched. With them sufficient for the day are the needs thereof. But such vegetative centenarians do not count, for it is most true that *non est vivere sed valere vita*. Old Parr, who, if he did not live to be 152 years 9 months, was probably at least a centenarian, retained his faculties to the end; all his organs were found healthy by William Harvey, who made the *post-mortem* examination. He might well have lived longer but for the transference from his healthy surroundings in Shropshire to London, and the different habits of life forced upon him when he became an object of curiosity to the Court. We believe Chevreul's death was hastened by the drums and trumpetings, as Sir Thomas Browne would say, with which enthusiasts came to celebrate his centenary, and we know that some admirers of Manuel Garcia were afraid that the festivities intended to mark his 100th birthday might have been changed into funeral obsequies.

It must be admitted that all the eager research that has been brought to bear on the question has scarcely touched the secret of longevity. The most exhaustive investigation of aged persons ever made is probably that made by Sir George Humphry for the British Medical Association. His conclusion, besides the importance of "temperance in all things," may be given in his own words as follows: "The prime requisite is the faculty of age in the blood by inheritance; in other words, that the body has been wound up, as it were, and sent into the world with the initial force necessary to carry on the living processes through a long period; that this is the case with every organ, and that the several organs are so adjusted to one another as to form a well-balanced whole."[1] In a word, if we may parody the famous explanation of the action of opium given by Molière's candidate for the doctor's degree, a man lives long *quia est in eo virtus longaevitiva*. A habit of long life is engendered somehow and is often transmitted to descendants. Therefore the most essential thing is to come of a long-lived ancestry.

Temperance in all things is a golden maxim, but men have lived to patriarchal ages who have persistently set this precept at defiance. Victor Hugo, who lived to the age of 83, did not smoke and he was moderate in his use of wine. On the other hand, he was a great eater. Theophile Gautier speaks of seeing his plate piled at the same time with "fabulous mixtures of cutlets, haricots cooked with oil, beef with tomato sauce, ham omelet, café au lait, with a little vinegar, a little mustard, and Brie cheese, which he devoured indiscriminately, very fast and very long." The gorge rises at it. The poet was intemperate in other ways, and we gather from his admiring biographer that he boasted of sacrificing three times to Venus at different altars in the same night. And this when he was 72. Hugo retained his Gargantuan appetite till his death at the age of 83. His intellect was vigorous to the end. The Homeric feats of Bismarck in eating and drinking made him noted even in a land of mighty trenchermen; he mixed beer and champagne, and ate all sorts of indigestible messes in quantities that would have killed an ordinary man. Yet he lived to be 84, retaining all the lucidity and strength of his understanding. These, of course, are heroic examples which do not invalidate the maxim that most men dig their graves with their teeth.

It must be owned, however, that the most perfect life from a hygienic point of view will not ensure longevity. A melancholy illustration of this is Sir Benjamin Ward Richardson, who gave it as his "fixed opinion" that every man should attain the age of 100. This was to be done by observing certain rules, which may be summarized as never to smoke

[1] British Medical Journal, March 10th, 1888.

or drink, to eat very little meat, to keep early hours, and so forth. Yet, within a few months of expressing the opinion we have quoted, Richardson, the sanitarily perfect man, himself died at an age a full generation short of that fixed by him as the limit of life attainable by all who followed his rules.

All the rules which have so far been formulated are summarized in the dedication of the famous *Regimen Sanitatis* of Salerno:

> Si vis incolumem, si vis te vivere sanum,
> Curas tolle graves, irasci crede profanum,
> Parce mero, coenato parum; non sit tibi vanum
> Surgere post epulas; somnum fuge meridianum;
> Ne mictum retine, ne comprime fortiter anum.
> Haec bene si serves, tu longo tempore vives.
> Si tibi deficiant Medici, medici tibi fiant
> Haec tria; mens laeta, requies, moderata diaeta.

All the "rules" are there, and till the elixir of life is discovered man will have to be satisfied with these general precepts.

REMUNERATION AND CONDITIONS OF SERVICE.

THE CHANCELLOR'S STATEMENT.

THE statement made by the Chancellor of the Exchequer to the Advisory Committee on October 23rd with regard to the remuneration the Government proposes to ask the House of Commons to authorize to be paid for medical services under the Insurance Act out of State funds will, we do not doubt, be read with great interest by all members of the profession. The text published in the SUPPLEMENT has been furnished to us by the Insurance Commissioners, and is full and official. It should be read in preference to the condensed statement made later in the day by Mr. Lloyd George in the House of Commons, because it sets out very clearly the course of reasoning by which he has been guided. It should be studied along with the Provisional Regulations for Medical Benefit published in the SUPPLEMENT a fortnight ago, for the two are interdependent, and it will be observed that the statement implies certain alterations in the Regulations.

The alteration of the terms of remuneration now proposed has brought about a new situation, differing materially from that which existed when the Representative Meeting resolved to break off negotiations with the Government. Upon this new situation the members of the profession must pass judgement. The meetings of the Divisions of the British Medical Association which will shortly be held will afford them an opportunity of discussing the matter and expressing their opinions. In coming to a decision they will have the help of the Report which is being drafted by the State Sickness Insurance Committee for presentation to the Council next Thursday. Immediately after that meeting the Council will issue its considered report, and meetings of Divisions will be held between November 4th and 16th. The meeting of the Representative Body has been provisionally fixed for November 19th and 20th.

Mr. Lloyd George's statement put the matter from his point of view, and he declared that throughout the whole course of the discussion of the scheme he has desired to treat the medical profession fairly. We can only wish that this intention had been made more evident to the profession at an earlier date; but we must now look at matters as they stand, and it will be recognized that on some grounds at least Mr. Lloyd George has modified his attitude so as to approach more nearly to the position desired by the profession. He admitted that the Plender Report was not by itself a sufficient basis for computing the amount which it would be fair to pay the profession under the conditions of the Insurance Scheme, for he had been convinced by the argument that at the present time a large number of people have no medical attendance at all, and that a still larger number receive inadequate attendance, but will in future receive regular medical attendance under the provisions of the Act. He also admitted that the claim for extras was just, but contended that they must be met, not by a special payment from the National Exchequer, but out of the proposed increase in the fund set aside for medical attendance. He also admitted that the criticisms of Regulation No. 28 with regard to remuneration were well founded, inasmuch as that Regulation proposed that the cost of drugs to an unlimited amount should be a first charge on the sum allotted for medical benefit. This, he said specifically, was obviously a very unfair arrangement. The proposal he now makes is to raise the amount originally proposed in the actuarial estimates for medical benefit from 6s. to 8s. 6d., to include the cost of drugs, appliances, and extras. Out of this amount 6s. 6d. a head would be allotted to the profession as a minimum payment for medical attendance, and 1s. 6d. would be allotted for drugs; this leaves 6d. a head, the distribution of which would be determined by the amount expended on drugs. The amount available for medical attendance in respect of medical benefit would therefore be anything from 6s. 6d. to 7s., subject, as we gather, to a deduction for mileage. He has also made an effort to meet the contention that during epidemics the doctor's work would be increased and his remuneration diminished, inasmuch as the expenditure on drugs would be increased. He proposes to form a central drug fund for such abnormal circumstances, but the amount and mode of distribution of this fund are left undefined. In addition he proposes that the treatment of tuberculosis, so far as it will be carried out by the general practitioner—including, it is to be presumed, domiciliary attendance and the filling up of forms—should be paid for on the basis of an inclusive fee; this fee would be 6d. a head of insured persons, deducted from the 1s. 3d. which the Act provides for sanatorium benefit, and would be in addition to the payment for medical attendance in respect of medical benefit.

It will be noted that the question of the mode of remuneration—whether by a capitation payment pure and simple, by a system of payment for attendance, or by a combination of these plans—is not here and now at issue. The Chancellor declined absolutely to contemplate a national income limit, but said that if the working population and the doctors in any given area liked to make arrangements of this kind they had a perfect right to do so within the terms of the Act. This, then, is a point which has been left to be settled by way of negotiations with the Insurance Committees in each area.

It would seem also that a similar result will follow from the failure to make specific provision for extras except major operations and specialist services. The Act contains no provision for institutional treatment, and for major operations and specialist treatment it would appear that the insured person will still be dependent on the voluntary hospitals, and nothing is said as to the payment of the staffs of such hospitals for services rendered to insured persons. A difficulty, perhaps, more immediately pressing arises with regard to the treatment of accidents—fractures, and so on; the treatment of such cases is anxious and exacting, involving serious responsibility, but an equitable adjustment of this difficulty seems to be left to local effort.

A great deal of the Chancellor's address was devoted to the consideration of the means which should be taken to provide a guarantee of a sufficient supply of drugs while preventing extravagant drugging. On the other hand, he sets out certain additional conditions which have been devised in order to improve the service. The first is that the medical profession shall undertake to give the medical certificates an insured person will require to enable him to get sickness or disablement benefit—a certificate that he is unfit for work, a certificate, when necessary, that he continues to be unfit for work, and a certificate that he has been restored to health. The second condition he makes is that the practitioners who act on the panels shall keep clinical records of the patients whom they treat. He admitted that this demand is new in respect of the industrial practice of this country, but undertook that the records required should be of the simplest character that would give the necessary information. He then went on to make the condition that the Commissioners should have power to see that a proper standard was reached and maintained, not merely in respect of the number of visits paid or the number of times a patient is seen at the doctor's surgery, but also that where necessary the practitioner "should resort to those modern means of exact diagnosis," the importance of which is increasingly recognized in the profession. "The modern means of exact diagnosis" is a very wide expression, and if all that it implies is to be carried out, it will be necessary for the nation in some way to provide clinical laboratories with an adequate skilled staff for carrying out the very tedious and complicated modern methods, such as, for instance, the Wassermann reaction. It will mean obviously an extension of the principle of municipal laboratories, linked, where possible, with universities or large hospitals which are already at work in some places for the diagnosis of diphtheria and typhoid fever.

It is to be regretted that the Government has not dealt more boldly with the question of mileage. It is left in an unsatisfactory state. The contention that because medical men in rural districts would probably receive the whole of the sum allotted for drugs should be taken as a set-off in respect of mileage will not bear examination. The cost of drugs and their dispensing has nothing to do with the cost of travelling. You cannot compare 4 lb. of tea with 4 o'clock. It would probably be better to establish a central fund for mileage. The total cost would not be great, and the risk of serious injustice to individuals would be avoided.

It will be seen that Mr. Lloyd George put in a strong light the advantages which some believe would result from the establishment of a National Medical Service, by which, we presume, is to be understood a whole-time service. We believe it to be impracticable, but the point need not be argued, for Mr. George has put the proposal aside.

We cannot pretend here to do more than mention some of the salient points which present themselves on a first perusal of the statement. It will require and receive the most careful and minute consideration from the State Sickness Insurance Committee and from the Council. But in a democratic body such as the British Medical Association the duty devolves upon every member to form his own opinion and to record it after discussion with his fellow members in Divisional meetings; it is most important that every member of the profession should do his utmost so to arrange his work that he shall be able to attend the meeting which will be held in his locality.

The new proposal falls short of what the profession has demanded in many important respects. But the justice of many of the claims put forward by the Association on behalf of the profession is recognized in principle. That this has been brought about is due to the firm stand made by a united profession against a scheme which it asserted, and which it is now admitted, would have inflicted a serious injustice upon it and would have rendered it impossible to provide the adequate and efficient medical service that is universally desired. The need for the maintenance of that unity is as great as ever.

PELLAGRA IN THE BRITISH ISLES.

The facts related in the article by Drs. Sambon and Chalmers on pellagra in the British Isles which is published in this issue will come as a surprise to many, and will open a new field to the alienist, the dermatologist, and the epidemiologist in Britain. That so serious a disease as pellagra has existed in these islands unrecognized for so many years is more than a surprise, although in this particular we are only repeating the experience of the United States of America, where it is now known that many thousands of sufferers from pellagra are to be found scattered about the country and domiciled principally in lunatic asylums, and where also the presence of such an enormous number of cases of this very grave disease has only been recognized within the last few years.

The paper will doubtless call the attention of the medical officers of lunatic asylums, poorhouses, and infirmaries to the clinical features of the disease, and it is not improbable that, as in the case of the United States of America, many cases will be recognized in Great Britain within a comparatively short time. History repeats itself, and it may well be with pellagra as it has been with myxoedema and many other "new" diseases which, when attention was called to their clinical features, were found to be common enough in the community. Of more importance than this, however, is the fact that another cause of insanity in this country has now been indicated, and, inasmuch as knowledge of the cause is a most important step towards the prevention of a disease, it is to be anticipated that some reduction in the incidence of insanity may be brought about.

Those who have followed the recent investigations by Dr. Sambon into the nature and cause of pellagra must know that in conseqnence of his study of the literature of the subject the inadequacy of the grounds on which rested the theory that the disease was due to the consumption of maize were made plain. The field studies of Drs. Sambon and Chalmers in many pellagra districts on the epidemiology of the disease have shown that its absence from towns and its occurrence only in the country in very circumscribed areas are circumstances incompatible with the maize theory.

The man who destroys an untenable hypothesis has done good work, but if in addition he gives us a new view of a very grave disease, and especially if he gives us a working hypothesis which may lead to a definite solution of the etiological problems involved, has conferred a great boon on medical science and on the public. This Dr. Sambon has done. The hypothesis he advances, founded partly on direct observation, topographical and epidemiological, and partly on analogy, is to the effect that pellagra is a germ disease, that the germ is a protozoal organism, and that this orgarism is conveyed by a special kind of insect, a *Simulium*. The very marked associa-

tion of the disease with streams haunted by the insect, the fact that the disease is believed to originate nowhere else than in the neighbourhood of such streams, that people coming from outside the endemic areas can acquire the disease during a very short visit to these areas, seem to support the view that *Simulium* may be the transmitting agent. The fact that the active clinical manifestations of the disease recur at fairly regular intervals, that the disease is not directly communicable from man to man, and a variety of clinical features similar to those associated with other protozoal diseases, are certainly compatible with this hypothesis. One remarkable circumstance Dr. Sambon has brought out is that, in a general way, in countries in which malaria and pellagra occur, the topographical distributions of these two diseases do not correspond. With occasional overlappings, where malaria is there pellagra is absent; and, conversely, where pellagra is present malaria is absent. Malaria, the mosquito-caused disease, is a disease of countries of stagnant waters, the haunts of the mosquito; pellagra is a disease of swift-flowing streams haunted by *Simulium*.

Though as yet unproved by direct observation or experiment, the hypothesis that pellagra is a protozoal disease communicated by *Simulium* seems so probable that it should be followed out. Now that attention has been called to the subject, we trust that a solution of a very important etiological problem will speedily be forthcoming.

RED CROSS AND RED CRESCENT.

THE honorary and other officers of the British Red Cross Society, including its active secretary, Mr. Frank Hastings, have been having an overwhelmingly busy time during the past fortnight, and do not seem likely to reach a period of comparative quiet until next Tuesday. By that date it is hoped that the last of the units whose dispatch is at present definitely in view will have started on its way out to the much scattered seat of war. Though the society is in theory only concerned with wars in which British forces are taking part, it has for some little time past foreseen that a demand for its activities might arise in the near East, and has been taking steps accordingly. Still there is a great difference between quietly arranging possible plans and actually sending out expeditions on short notice. However, it is getting through its work with remarkable speed and completeness; one unit has already been dispatched, and it is hoped that five others will be ready to leave within three days or so of the publication of this issue. An interesting feature has been the rush of volunteers. The number of applicants has been so large that the task of giving each of them an opportunity of stating his claims to consideration has been no small one. Many factors besides professional knowledge and ability go towards the making of an ideal Red Cross worker, and those finally selected as the most likely among the candidates are all men in the prime of life and of excellent physique. This is an essential point, since before the various units return their members are pretty certain to have to undergo very considerable hardships. The general work of selection has been carried out by a committee, the final choice between two candidates of equal merit having usually been made by Sir Frederick Treves and Sir Anthony Bowlby, both of whom have, as is well known, ample personal experience of the needs of the situation. Three of the units are to make their way to Turkey, two to Greece, and one —that which has already left—to Montenegro. Each consists of 3 medical officers, 3 dressers, 5 nursing orderlies, 5 general duty orderlies, a cook and a sergeant. Though nearly all the medical officers are young men, some are sufficiently old to have already specialized in surgery, and all have had recent and ample experience of hospital work as residents or otherwise; some, too, have had experience of a not essentially different kind from that now in anticipation. One of them is Dr. Martin Leake, who gained the Victoria Cross as a civil surgeon in South Africa, and who until recently was resident in India as chief surgeon of the Nagpur Railway. Another is Mr. C. M. Page, resident assistant surgeon at St. Thomas's Hospital; a third is Dr. A. B. Bradford, who until a little time ago was resident medical officer at St. Thomas's Home, and before that had filled all resident appointments in the hospital itself; a fourth is Dr. Goldsmith, until recently a medical officer in New Guinea. Others are Messrs. H. L. Mann, Bourdillon, Anderson, Thornton, Ward, Appleyard, Steele, and Gardner. Each unit is being provided with the equipment of an ordinary field ambulance, but not with beds and tents. The primary need in the Balkans is the presence of more surgeons and nurses, and it is intended that each unit shall get as near to the front as possible. At present there is no true front, and to render the greatest service possible the units may very well have to make frequent moves. Later on, when the war has become in some degree localized, beds and tents or huts may be sent out if this seems desirable and if funds are forthcoming. Meantime the units will be able to do most valuable work, utilizing cottages and other buildings placed at their disposition. The cost of equipping and dispatching each unit is estimated at £1,500, while its maintenance will entail an outlay probably of some £850 a month. The cost of provisions is sure to be very high, and when this factor, and the employment of interpreters, who are certain to be required, are taken into consideration, these estimates may well be considerably exceeded. In any case, a large amount of money is certain to be absorbed, and will have to be found by the general public, since the ordinary funds of the society are not available for the purpose. It is to be hoped that it will be found, because the medical arrangements of none of the forces concerned are in the least commensurate with the vast numbers of men who have been summoned to join the colours. Moreover, there is certain to be an immense number of wounded persons having no direct call on the services of the medical officers of any of the combatant forces, since on this occasion all the worst features of war between regular forces and of a civil war are combined. All the units operating in each of the main areas are in charge of a Director appointed by the society to attend to the business side of the work, such as the payment of expenses, securing provisions and negotiations with the authorities. One of them is Surgeon-General Bourke, C.B., who has had wide experience of war in various parts of the world; another is Major Doughty Wylie, C.M.G., who, when in the consular service, did notable work for the victims of a massacre a few years ago in Caramania, and a third is Colonel Delmé Radcliffe, C.V.O., C.B., the officer who, as Military Attaché at Rome, organized the relief operations undertaken subsequent to the Messina earthquake.

THE WASTE OF MEDICAL EXPERIENCE.

WHEN an old practitioner who has for many years been wrestling with disease in all its forms passes away, one cannot help a feeling of regret at the irreparable loss of the experience which dies with him. Books we have in abundance and superabundance, and the reports of cases which fill our columns and those of other journals generally contribute something to the sum of knowledge or help in the extinction of error. Without such records progress would be impossible, and even discounting the natural enthusiasm of the pioneer, every case honestly reported adds a stone to the slowly rising temple of truth. But what would we not give for the fruits of the ripe experience of a man like Jenner or Gull, who are

represented in catalogues by a few papers and addresses! Something of this, of course, they gave those who had the privilege of being their pupils, who in turn may hand it on to their pupils, until it becomes a floating tradition which influences generations to whom the original source of inspiration is only a *magni nominis umbra*. What would we not give, too, for the experience of a man who had been in general practice for half a century, who had seen many whom he had brought into the world grow up and in their turn become parents and then pass away, all the time under his observation! Such men seldom have time to write down what they see and what their practice has taught them. Often, too, they have not the faculty of exposition. They are like the inarticulate poets who die "with all their music in them." Generally, however, such experience is so personal to the man, consisting as it does of the accumulated impressions of all the senses gathered by long observation, that by its very nature it is not transmissible to another. As Baglivi said, *Multa sunt in praxi quae nec dici nec scribi possunt*. It was in this that the old system of pupilage had an advantage which modern scientific training, with all the apparatus of the clinical laboratory, fails adequately to supply. There is the physiognomy of disease, the interpretation of which has, owing to the large use of instruments of precision, become almost a lost art. There is the knowledge of that subtle something, called for convenience "constitution," which so often makes all the difference between success and failure in treatment, and other still more intangible things, which together make up the knowledge a man acquires in practice. It is true that the records might sometimes be disappointing, as in the case of the celebrated Dr. Jephson of Leamington, who was reputed to have made £12,000 a year by a practice which included patients from the most distant places. The legend is that when his case-books came to be examined after his death, the entry against most of the names was the simple formula—"wind and fat." The young man writes because he thinks he has something new to tell; as he gets older he probably writes less, as Syme's *Surgery* became smaller in each successive edition. The old man does not write because he finds age has made him indolent and he is perhaps unwilling to put his experience on paper. Indeed, he would not, for the fear of scandal, care to record all his experience. Yet if old practitioners could and would set down faithfully what they had seen and done in their professional career, what a mine of lore not to be found in textbooks or scientific monographs we should have! We certainly do not wish to suggest that every doctor should write his experiences, but there are a few in every generation from whom such a record would be a boon of permanent value.

THE HEALTH OF FEMALE CIRCUS PERFORMERS.

There has been for a good many years past much discussion about the evil effects of excessive brain-work on girls, and now a disposition is becoming evident to maintain that the gymnasium and the playground are being abused. Dr. Angenette Parry[1] has recently read before an American medical society a paper on the relation of athletics to the reproductive life of woman. Educational institutions have been freely canvassed by Dr. Parry, and though she has received, as might have been expected, very contradictory reports, the opinion seems general that competitive athletics—that is, training for foot races, "tournaments," etc.—are, on the whole, bad. "I personally know two or three stunning girls who have wrecked their health by over-exercise," says "a very intelligent layman." On the other hand, it is certain that neither young women who "run to muscle" nor psychically masculine women are always barren. How far gymnastics and games are favourable or otherwise to the development of the uterine, pelvic, and perineal musculature we cannot say; at least Dr. Parry's evidence is conflicting. One point seems clear, and that is that the evils of athletics in the case of school girls in high-class institutions are largely due to over-luxurious surroundings in early child life and the resultant debility and neuroses. Amongst girls used to physical exertion from infancy, and free from the ill effects of improper mental training, professional athletic exercises seem to have, on the whole, a favourable effect, not only on their health, but also on their sexual system. As a rule, they are not subject to menstrual disorders. They are by no means infertile. Among professional women athletes abortion is not rare, but it appears to be almost always due to a fall. Lastly, as a rule, the breasts of these subjects secrete "plenty of milk." A bare-back rider, 35 years old, accustomed to making fifty jumps in six minutes through hoops, menstruated first at the age of 16. She married at 17, and early marriages are common amongst women of her class. She is subject to slight dysmenorrhoea, which antedated marriage, but she goes on with her work. She feels headachy and dizzy, but her head clears directly she enters the ring. This latter piece of evidence is interesting in respect to the physiology and psychology of menstruation. Four times pregnant, she miscarried after a fall on two occasions, but she rode until the eighth month in her second pregnancy, and bore a child to term, which weighed 8 lb. at birth. The first pregnancy ended in a miscarriage, and the third in abortion, after an accident; during the fourth pregnancy she rode to the eighth month, and the child was born at term. Several other circus performers furnished Dr. Parry with equally satisfactory reports. These women wore no heavy skirts or corsets, and their career began in childhood. The trapeze performers and "hand balancers" seemed equally sound as females. They seemed free from neuroses, and professional selection must here be taken into account. Nobody would train a nervous little girl for the trapeze.

[1] *Amer. Journ. Obstet.*, September, 1912, p. 341.

ALCOHOL AND MODERN LIFE.

In an address at a conference of the National Unitarian Temperance Association, Mr. J. Lionel Tayler, M.R.C.S., maintained that stimulants do not, like narcotics, among which he included alcohol, create a craving. Tea, coffee, and kola, he admitted, might form a habit, and on breaking the habit a feeling of flatness and malaise might follow, but with narcotics it was otherwise; they alone were associated with moral deterioration, and were, in fact, devolutional. They held mankind back from the upward evolutionary path. He considered, therefore, that they must all be abandoned. Mr. Tayler stated his views with a candour and moderation all too rare with writers on the alcohol question. He led logically to his main conclusion by a review of the action of narcotics and the changing social conditions of modern life, and did not evade the obvious answer to his Utopian conclusion—the fascination narcotics have for millions of civilized and uncivilized people. In spite of the penalties of indulgence, no race has yet been discovered that does not consume them either to banish care, to plunge into forgetfulness, however temporary, to create a flash of intellectual excitement, or in the more refined civilizations because they are agreeable to the palate. The use of narcotic substances is ingrained in mankind; and, until human nature is altered, it seems unlikely that the present or any near generation will see their use altogether die out. Mr. Tayler gives a very useful bibliography, and deplores, as every one must, that death certificates omit mention of alcohol, save in the flagrant instances where it is impossible to ignore it. In answer to his plea for research over the whole narcotic field, it may be pointed out that the Society for the Study of Inebriety exists for this very purpose.

CAN THE UNDELIVERED FETUS CRY?

The paragraph on this subject which appeared in the Journal of September 14th, page 653, has been read with interest by many, and three interesting communications, by Dr. J. Reidy, Mr. A. Teevan, and Dr. Oliver Smithson, appear in our present number. Over fourteen years ago Dr. Charles Kevin of Belfast related a case of intrauterine pulmonary respiration in the Journal (May 16th, 1898, page 1258). He vouched for the fact that the child, in a labour which he attended, cried and sobbed for two hours before birth. The membranes had ruptured, and Dr. Kevin states, "I could feel the apex of the child's left ear by sweeping my finger round the os." Some hours later he heard "a distinct sobbing, smothered cry." In Dr. Reidy's first case the child, it is stated, cried while internal version was being performed. In Mr. Teevan's case the cries were heard after a female catheter had been passed up between the uterine wall and membranes, to induce labour at the eighth month. In Dr. Reidy's second case cries were uttered when the child's head was in the vagina. Dr. Smithson heard cries, audible to the mother and nurse, after he had applied the forceps. Thus there can be no doubt that in all these instances sounds were heard before the child was born. In four, and probably in all, air had entered the genital canal. The sound was produced with the aid of the air, that is clear, but though many obstetricians believe that the fetal lungs make the sound, while others, like Sippel (whose experience was recorded in our recent paragraph), insist that the "cries" are due to expulsion by uterine contractions of the air admitted to the genital canal.

THE KING AND THE LONDON SCHOOL OF TROPICAL MEDICINE.

Mr. Austen Chamberlain has announced that the King has been graciously pleased to mark his interest in the London School of Tropical Medicine and his care for the welfare of his tropical dominions by sending a contribution of £100 to the fund opened by the Lord Mayor for the purpose of the school. Mr. Chamberlain says that His Majesty's interest and approval encourage the committee formed in the City with the aid of the London Chamber of Commerce to make a renewed effort to secure the £100,000 for which they originally asked. The Lord Provost of Glasgow and the Lord Mayor of Bristol have promised their assistance. Already nearly half the sum has been secured. Mr. Chamberlain points out that more is urgently needed to place the school on a sound financial basis and to enable it to prosecute effectively the great work of training and research by which the deadliest terrors of tropical life are being steadily eliminated, the death-roll of whites and natives alike is being diminished, and the development of these fertile lands is rendered possible without the terrible suffering and loss of life which it has previously entailed. He commends "this truly Imperial work to the favourable consideration of all who are interested in Imperial development or tropical trade." Since the first small beginnings of the school we have not ceased to call attention to the vast importance, from a national as well as a scientific point of view, of the work which it was established to do. It has done much both in the way of teaching and discovery, but it could accomplish far more if it received the increased support for which it is asking. The hindrance to colonization and to trade caused by ignorance of the means of combating tropical disease is slowly but surely becoming appreciated by business men and even by politicians. We earnestly hope that the appeal, which is patriotic in the highest sense, will receive a prompt and satisfactory response. The King, in the letter enclosing his cheque, refers to the fact that the School owes its inception to Mr. Chamberlain's father, an expression of interest which must be highly gratifying to him. Donations to the fund may be sent to the Secretary of the School of Tropical Medicine, Seamen's Hospital, Greenwich, or to the Right Hon. Austen Chamberlain, 2, Egerton Place, S.W.

At a special meeting of the Council of the Royal College of Surgeons of England, held on October 17th, Mr. Edmund Owen was elected vice-president in the vacancy caused by the death of Mr. Clinton Dent.

Medical Notes in Parliament.

[From our Lobby Correspondent.]

NATIONAL INSURANCE SCHEME.

MEDICAL REMUNERATION.

Statement by the Chancellor of the Exchequer.

Sir Philip Magnus asked the Chancellor of the Exchequer whether he could now state what additional sum he was prepared to recommend Parliament to set aside for the provision of medical benefit under the National Insurance Act; and, if so, whether he could state what would be the normal amount offered for each insured person, with or without drugs, and what arrangements had been suggested for securing, where necessary, exceptional medical or surgical treatment.

Mr. Lloyd George: After careful consideration and consultation with the members of the Advisory Committee, the Government have decided to increase the sum available for medical benefit to an amount which they are satisfied will secure an improved medical service for the industrial population, and at the same time afford an adequate remuneration to the medical practitioners for the work which they will be required to do under the Insurance Act. The House will remember that, on the second reading of the bill, I stated that there was a margin which would have enabled an increased amount to be paid to the doctors where a case was made out. But during the passage of the bill provisions were inserted, particularly as to the free choice of doctor and the system of providing drugs, which tended to increase the cost of medical benefit, while other charges had at the same time absorbed the margin originally available for these purposes over and above the 6s. assumed in the actuarial estimates. The Government have always recognized that, as they could not throw the burden on the societies, these charges would necessitate an additional contribution from the Exchequer. At the same time, the Government recognize the reasonableness of the demand that the medical profession should receive a guaranteed minimum rate of remuneration without trenching upon the sum necessary for an adequate supply of drugs. It was always contemplated also that provision must be made for extras, including mileage. This cannot, however, be unlimited, and provision for it is included in the addition which it is now proposed to make. The financial proposals which the Government intend to submit to the House may be briefly summarized. It is proposed to add to the amount already available for medical benefit a further sum equivalent to 2s. 6d. per insured person per annum. This will enable the provision for medical benefit to be raised to 8s. 6d. a head for insured persons, with a small additional sum to be reserved for extra drugs required at times of excessive

sickness. Of this amount it is proposed that a minimum of 6s. 6d. should be definitely assigned for medical remuneration and 1s. 6d. for drugs. A further 6d. would be available for drugs where required, and will otherwise be available for medical remuneration. Counting a further 6d., which will be applicable from the sanatorium benefit fund for the treatment of tuberculosis by general practitioners, the total remuneration which the doctors will receive (exclusive of drugs) will amount to 7s., or possibly 7s. 6d., per head of the insured. This additional grant, the total annual cost of which is estimated at £1,650,000, will be made under such conditions as to secure enhanced efficiency of medical service, and that the doctors will give such certificates as are necessary for sickness and disablement benefits, and will keep such simple records of disease as are requisite. I am publishing at once a full statement of the effect of these proposals.

Lord Helmsley: Does the 8s. 6d. include mileage, or is there a further sum?

Mr. Lloyd George: Yes, it is an inclusive fee.

Mr. Ginnell: Will the right hon. gentleman say how this alteration will apply to Ireland?

Mr. Lloyd George: Irish members of all sections deliberately pressed me to leave medical service out.

Mr. Barnes: Do we understand that the certificates are to be given free by the doctors?

Mr. Lloyd George: Yes.

Mr. Ginnell: Cannot the right hon. gentleman apply some proportion of this further contribution to Ireland? (Cries of "No.")

Mr. Ward: Does this mean that a proportion of the sum, amounting, I think, to nearly 3s., which it was expected at the time the bill was passed through the House might be available for extra benefits, is now taken entirely by the doctors?

Mr. Lloyd George: No; that is exactly what I explained—that that margin will not be trenched upon at all.

Mr. Lane Fox: Will this require a special supplementary estimate?

Mr. Lloyd George: Oh, yes.

Mr. Clynes: In the event of this announcement not having the desired result, does the right hon. gentleman contemplate any organization of a State medical service?

Mr. Lloyd George: I think my hon. friend had better wait until he sees the full statement, because I deal with that point.

Mr. Lardner: Will Ireland not be entitled to a share in the increased grant in regard to sanatorium benefit? (Unionist cries of "Home Rule.")

Mr. Lloyd George gave no reply.

[The full statement made by the Chancellor of the Exchequer to the Advisory Committee earlier in the day is printed in the Supplement.]

QUESTIONS.

Sanatorium Benefit.

In reply to Captain Faber, who asked what steps had been taken towards finding a consumptive sanatorium for two applicants, Annetts and Merriott, who had applied under the terms of the National Insurance Act, Mr. Masterman said he was informed by the Hampshire Insurance Committee that Mr. Annetts applied for sanatorium benefit on October 4th. The county medical officer of health wrote to the applicant the same day to inquire whether he wished to go to a sanatorium, and received a reply on October 7th, from which it was understood that the applicant was going to Salisbury Infirmary. A further letter was, however, received from the applicant, from which it appeared that he had not gone to the infirmary, and his case was under consideration. The Royal National Sanatorium at Bournemouth had agreed to accept Mr Merriott, and hoped to be able to admit him in about a fortnight's time.

Captain Faber then asked was it the policy of the Government to shunt on to convalescent homes and hospitals what was really the duty of the Act. Mr. Masterman replied that the Royal National Sanatorium was a fully recognized sanatorium by the Local Government Board, and the Insurance Committee would pay for the man going there. In reply to a further question he said that the Salisbury Infirmary was not recognized under the Act. Now that the Insurance Committee knew that the man was not going to the infirmary, it was considering what sanatorium to send him to.

Medical Benefits (Fishermen).

In reply to Mr. Fell, Mr. Masterman said that the whole question of the administration of medical benefit in the Highlands and Islands was now being investigated by a Departmental Committee. He was unable to make any announcement as to the details of the arrangements in these districts in anticipation of the report which would, he hoped, be issued at an early date.

Medical Fees.

Sir Arthur Markham asked the Secretary to the Treasury whether he had sanctioned Clause 28 of the Regulations, lately issued by the Insurance Commissioners, which provided for the purpose of determining the amount of remuneration payable to medical men, that the committees should determine the total amount to defray the cost of medical benefits, and should deduct therefrom certain specific charges, and that the sums remaining should be paid to medical men; and whether, in view of the declared policy of the Government that insured persons should have the benefit of having the best drugs irrespective of cost, he would say whether, if under this Regulation, a medical man prescribed expensive drugs, his salary would be reduced accordingly. Mr. Masterman said that the draft Regulations now before Parliament could not be interpreted except in connexion with the general question of remuneration for medical benefit under the Insurance Act. This question was still under the consideration of the Government, and the Chancellor of the Exchequer hoped to make an announcement on the subject within a few days.

Maternity Benefit.

Mr. Charles Bathurst asked whether the Health Insurance Commissioners had issued or proposed to issue provisional regulations for the uniform administration of maternity benefit under the National Insurance Act, or whether it was open to each Insurance Committee to make its own arrangements and regulations in reference thereto. Mr. Masterman said that the Commissioners were not authorized to issue such regulations, but they proposed to issue model rules for the administration of benefit by Insurance Committees to deposit contributors. The maternity benefit of members of approved societies was administered by the societies.

Tuberculous Patients in Ireland.

In reply to Mr. Guiney, Mr. Masterman said that provisional arrangements for the treatment of tuberculous insured persons had been made by the Insurance Committee of each county and county borough in Munster. Seventeen persons had been recommended for sanatorium benefit by the several committees. All these seventeen persons had been actually sent to institutions for treatment, the numbers from each Insurance Committee being as follows: Clare County, two; Cork County, one; Cork County Borough, one; Kerry County, five; Limerick County, two; Limerick County Borough, two; Tipperary South Riding, two; Tipperary North Riding, one; Waterford County, one.

Sanatorium Grants (Ireland).

Mr. T. M. Healy asked the Chief Secretary for Ireland whether the share of the county and borough of Cork of the one and a half million granted for sanatoriums on a population basis was £13,182; whether only £9,000 was proposed to be allotted; how much of the £145,000 given to Ireland was the Women's Health Association getting for the erection of the sanatorium at Peamount, county Dublin; on what principle was this allocation made; and were counties like Cork, which could derive no benefit from Peamount, to be deprived of their due proportion of the sum provided by Parliament. Mr. Birrell said if the whole £145,000 were distributed between counties and county boroughs in proportion to population, the share of Cork county and borough would be approximately £13,000. The share of the county and borough in the sum of £120,000 at present available for distribution on this basis was £10,750, and it was proposed to allot this amount immediately. A further sum would

be allotted at a later date when the amounts to be charged against the shares of counties which have entered into contracts with the Women's National Health Association, instead of making provision for themselves, had been ascertained. By this means such part of the £25,000 originally allotted to the Women's National Health Association as was expended on behalf of particular counties would be recovered from the shares of these counties, calculated on the population basis, and would be available for distribution on the same basis as the £120,000 now being allotted, leaving only the remaining portion of the £25,000 (which would represent expenditure incurred in the interest of the country generally in respect of temporary provision for the counties which were contemplating building sanatoriums for themselves and in respect of accommodation whether for adults or for children which would be available in the future for any county insurance committee which might apply for it), as a final charge against the £145,000 grant as a whole.

Peamount Sanatorium.

Sir John Lonsdale asked the Chief Secretary for Ireland if he was aware that, although thousands of pounds of public money were being spent by the Women's National Health Association upon the Peamount Sanatorium, the water supply of the place had been found to be insufficient in quantity and unfit for dietetic purposes; would he state who was the medical inspector of the Local Government Board who inspected the site and reported it to be suitable for a sanatorium, and why the inspector did not satisfy himself that there was a sufficient supply of pure water for a large institution before recommending the site; would he state approximately how much money had been spent in endeavouring to find water at Peamount; and whether he now considered that this sanatorium was fit for the reception of patients, seeing that all drinking water had to be carted a distance of two miles. Mr. Birrell replied that the Peamount site was selected by the Women's National Health Association after it had been examined and most favourably reported upon by a well-known Dublin firm of architects and sanitary engineers and subsequently inspected by three eminent Dublin physicians who endorsed and confirmed the report. The Local Government Board was not asked to send a medical inspector prior to the decision to select the site; but ever since the actual commencement of works of construction these had been frequently inspected by officers of the Local Government Board, and no payments from public funds had been made except after being duly submitted to the Board and authorized thereby. The water supply, on investigation, was found unsuitable for dietetic purposes, but the sinking of a well which would yield, it was believed, an adequate supply of water for all purposes was now progressing. The sanction of the Local Government Board to the opening of the institution as a whole would be given when a thoroughly satisfactory water supply for a large establishment of the kind had been obtained, and not before; but owing to the urgency which was the primary consideration for the establishment of the sanatorium, the Board's Inspectors had felt justified in recommending that during the sinking of the well a limited number of patients should be admitted for whom a supply of drinking water of the most excellent quality could be provided from a spring outside the property, the necessary arrangements having been made for this purpose. He was informed that the expenditure incurred in connexion with the water supply at present was well under £200. He believed the spring was some two miles distant, but it would not be the sole source of supply. The sanatorium would not be certified as a fit and proper place until there was an ample and sufficient water supply which could be used for all proper purposes and at once. The Local Government Board advisers did not report as the site had already been properly examined to the satisfaction of the Board by experts in Dublin. This supply of water from the well was no doubt insufficient for the sanatorium as a whole, although there was no difficulty whatever in obtaining it, and it was by all accounts about the best water that could be obtained in the district. The Peamount Sanatorium would not be certified to its full amount until the difficulty of the water supply had been got over, if that had not already been done.

Hospital Nurses (Ireland).

In reply to Captain Craig, the Financial Secretary to the Treasury said that the nuns employed by the New Ross Union were not at present excepted, and their insurance cards for the last quarter had been duly stamped. The union was now in communication with the Commissioners with a view to the exception of their officials (including these nuns) under Schedule I, Part II (*b*). Such exception, if granted, would depend on the terms of their employment under this union, and would afford no ground for a general exception of all hospital nurses in Ireland, which the Commissioners had no power to grant under the Act.

The Mental Deficiency Bill, which is one of the bills that the Government must pass if possible, still pursues a somewhat eventful career. On Thursday, October 17th, when it was before Standing Committee B, there were some twenty-eight pages of amendments on the paper. The Home Secretary's new clause setting up a board of control as the central authority to deal with lunatics, the feeble-minded, and other mentally deficient persons, was the subject of discussion. An amendment to increase the legal members from four to six was defeated, but one opening the position to solicitors was accepted. The words requiring one at least of the Commissioners to be a woman were objected to, but eventially they were accepted. It was then moved that two of the paid Commissioners should be women. Mr. McKenna said he was willing to accept words which would secure that one of the paid Commissioners should be a woman and one of the unpaid members also. He informed the Committee that, notwithstanding what happened at the last meeting, he did not propose in the first instance to appoint more than eleven paid Commissioners—that was, three in addition to the existing eight Lunacy Commissioners, who would form the original board. This amendment as suggested by the Home Secretary was finally adopted as follows:

> There shall be constituted a Board of Control consisting of not more than fifteen Commissioners, of whom not more than twelve shall be paid Commissioners; and of the paid Commissioners four shall be legal Commissioners—that is to say, practising barristers or solicitors of at least five years' standing; and four at least shall be medical Commissioners—that is to say, duly qualified medical practitioners of at least five years' standing; and at least one of the paid and one of the unpaid Commissioners shall be a woman.

A proposal to limit the appointments to five years was rejected, and then the Committee adjourned.

At the meeting of the Standing Committee on October 22nd, the constitution of the Board of Control was further discussed, and an amendment was moved by Captain Jessel providing that the three unpaid Commissioners should be appointed by the Home Secretary, and not by the Lord Chancellor after consultation with the Home Secretary as proposed in the bill. Mr. McKenna resisted the amendment, and said that he was anxious not to encroach on the powers of the Lord Chancellor. Under the arrangement proposed in the bill the Home Secretary accepted the responsibility to Parliament by appointing eight of the fifteen members of the Board. A good deal of discussion ensued, but eventually the amendment was carried by 25 votes to 6. After this Government defeat, the new clause of the Home Secretary was added to the bill as amended. The Chairman, it was decided, should be appointed by the Home Secretary after consultation with the Lord Chancellor, and it was decided to call the new body simply the Board of Control. Mr. McKenna then accepted an amendment setting up an administrative council consisting of the Chairman and four members of the Board. This would be a temporary arrangement to secure parliamentary control while the Lunacy Commissioners still held office. As regards the prospects of the bill, the Home Secretary announced that it was the firm, resolute intention of the Government to pass the bill this session, but as it contains sixty-eight clauses its prospects become less favourable at each meeting of the Committee.

Medical Practitioners (Income Tax).—On Monday last, in answer to Mr. Cawley, the Chancellor of the Exchequer stated that it was not possible to give any estimate of the amount of income tax paid by members of the medical profession in respect of professional earnings. The information was not available.

England and Wales.

[*FROM OUR SPECIAL CORRESPONDENTS.*]

LIVERPOOL.

TOWN PLANNING AND HOUSING EXHIBITION.

AT a meeting held at the Town Hall on October 18th, a scheme was proposed for holding a town-planning and housing exhibition at the beginning of next year. The estimated cost is about £750, and it is proposed to raise £500 by subscription and to form a guarantee fund of the same amount. The Chairman (Sir W. Lever) said that the exhibition would show what was being done in other parts of the world in town planning. There was no more fitting centre for such an undertaking in the whole of the United Kingdom than Liverpool, which in housing reform, sanitation, and the development of civic life on the highest plane had always taken the leading part. Much of the credit of this was due to Dr. Hope. Liverpool had been extremely fortunate in its medical officer of health, who had carried on the work under considerable difficulties. Liverpool ought eventually to become the model of the provinces in this respect. On town planning depended the future of Liverpool and its health and prosperity. Professor Adshead said that it was fitting that this educational effort should be made in Liverpool for various reasons, one of which was that a chair of town planning, the only one in the country, had been founded in the university by Sir W. Lever. The exhibition would show the historical growth of Liverpool in its present-day aspect, and the plans for its future development. Apart from this there would be exhibited a large and valuable collection of town-planning material from the Royal Academy.

ASSOCIATION OF MEDICAL OFFICERS OF HEALTH.

It is a pleasure to record that Dr. Hope, the medical officer of health for Liverpool, has been installed president of this association. His qualifications for the post are many. He has worked sedulously for the removal of slum areas in his city. Not only has he sought to change the abodes, but has also aimed at changing the lives of the people, and each year he has been able to point to something accomplished.

ANNUAL MEDICAL SERVICE AT ST. LUKE'S.

The annual medical service was held at St. Luke's Church, Liverpool, on Sunday, October 20th. Archdeacon Maddon conducted the service, the Rev. J. S. Cooper, M.A., read the special prayer, and the Lord Bishop of Liverpool the lesson. The sermon was preached by the Right Rev. A. Robertson, D.D., Bishop of Exeter, who founded his discourse on St. Mark ii, 17: "When Jesus heard it, He saith unto them, They that are whole have no need of the physician, but they that are sick: I came not to call the righteous, but sinners to repentance." The offertory was taken in aid of the British Medical Benevolent Fund. The service was well attended by members of the medical profession as well as laity.

NATIONAL VIGILANCE ASSOCIATION.

International Bureau for the Suppression of the White Slave Traffic.

The annual meeting of the Liverpool branch of the National Vigilance Association was held in the Town Hall on October 18th. The was a large attendance. The Rector of Liverpool, who was in the chair, said that not only in Liverpool, but all over the country, as evidenced by the meetings held in London and elsewhere, interest had been shown in this matter, and both Parliament and people were keenly alive to the great danger of the white slave traffic.

Miss Edith Rose, the honorary secretary, reported that 835 girls were met at the Liverpool landing-stage by the organization, which had discovered that girls were being brought into Liverpool. That had been stopped, at all events, as far as the landing-stage was concerned. It was found also that girls were being taken out of the country. The association had made searching inquiries, and had received valuable information relating to the traffickers, which they forwarded to the police. One of the persons engaged in this traffic had remarked that it was getting so difficult to bring the girls through Liverpool that another way must be found.

At the conference this week at Brussels a proposal of considerable importance from the English committee is that which recommends that all the foreign women of the unfortunate class shall be repatriated when a charge is brought home to them. It is argued that in getting rid of the foreign women of this class the foreign criminals who live upon them would also disappear. Germany is proposing an application of the repatriation laws for very similar reasons.

MANCHESTER AND DISTRICT.

THE MIDWIVES ACT IN MANCHESTER.

IT is uncertain as yet how far the working of the Midwives Act in Manchester may be modified by the administration of maternity benefit under the Insurance Act, as it is probable that a very large proportion of the women dealt with under the Midwives Act will be either insured themselves or the wives of insured persons, and so will be eligible for maternity benefit. At present the town council pays the fees of medical practitioners summoned to the assistance of midwives in the case of women whose family income is below a fixed limit, but when maternity benefit comes into force such fees will be payable out of the insurance funds under Clause 18 (1) of the Act, and in this way there will be a saving to the city rates of several hundred pounds a year. Thus the figures given in the report of the Midwives Supervising Committee are of interest in connexion with maternity benefit.

During 1911 there were 18,583 births registered in a population of 716,734. This is a birth-rate of 25.93, which is the lowest on record. Of these, 10,937 were attended by midwives, and, of the remainder, most were attended by general practitioners, though probably some only received attention from neighbours and relatives, and a considerable number of births occurred in institutions, such as the workhouse infirmaries. The number of cases in which medical practitioners were summoned to the assistance of midwives in accordance with the rules of the Central Midwives Board was 2,729, of which 1,514 were in the private practice of midwives and 1,215 were in connexion with the various lying-in charities. It is noted that medical aid has been advised more frequently in cases of retained placenta, perineal tear, feeble or premature children, and inflammation of the eyes. In each case where a record was received of rise of temperature, rigor, offensive lochia, abdominal pain, mastitis, or other suspicious symptoms, immediate investigation was made. Out of the 2,729 cases where medical aid was summoned the practitioners made a claim on the corporation for the agreed fee in 595 cases. These were considered by the Medical Subcommittee, which recommended that payment should be made in 506 cases, the total sum paid amounting to £471 1s., compared with £425 7s. in the year 1910. Of the 89 cases which were rejected as not fulfilling the conditions, in 50 the family income was above the scale, while 36 did not fulfil in various respects the conditions under which a fee is paid, and 3 were referred to boards of guardians. The income limit below which the corporation pays the fees of practitioners summoned by midwives varies with the size of the family from 21s. to 33s. a week, and as in all probability most of the 50 cases that came above this limit were of the working classes who will be insured, it seems likely that if maternity benefit had been in force the approved societies which administer the benefit would have been liable to pay for these cases which the corporation rejected.

During the year 135 cases of puerperal fever were notified, of which no fewer than 24 occurred after abortion or premature labour. The date of onset of the fever was in 88 cases within the first four days after delivery, while in 39 cases the attack began from the fourth to the tenth day, and in 8 on or after the tenth day. In 44 cases midwives alone were present at the confinement, in 63 doctors alone, and in 28 both doctors and midwives were present. During the year two special nurses were appointed to assist medical practitioners and midwives and to attend to the mothers and infants, and also two other special nurses to visit cases of ophthalmia neonatorum.

BIRMINGHAM.

WORK OF THE HOSPITALS IN 1911.

THE following list shows how large was the number of patients treated in the Birmingham hospitals in 1911. The total cannot be taken as the actual number of patients attending the hospitals, as there are a certain number of patients suffering from chronic or incurable diseases who wander from one institution to another, and also some of the in-patients are probably included also among the out-patients. Even taking this into consideration, there can scarcely have been less than a quarter of a million patients treated in the Birmingham hospitals during 1911. It will be noticed that in every hospital there was a deficit on the year's working.

	In-Patients.	Out-Patients.	Total.	Income.	Expenditure.	Deficit.
				£	£	£
General Hospital	5,252	73,309	78,561	22,006	27,177	5,171
Queen's Hospital	3,172	39,743	42,915	11,234	16,251	5,017
General Dispensary ...	—	63,414	63,414	11,485	11,762	277
Children's Hospital ...	1,023	14,589	15,612	5,579	5,867	288
Eye Hospital...	1,311	32,526	33,837	7,836	8,659	823
Women's Hospital... ...	1,032	4,037	5,069	5,607	8,486	2,879
Maternity Hospital ...	437	1,435	1,872	3,311	3,915	604
Orthopaedic Hospital ...	314	2,242	2,556	2,392	3,387	995
Skin and Urinary Hospital	185	6,768	6,953	2,697	3,073	376
Ear and Throat Hospital...	1,430	6,871	8,301	3,047	3,276	229
Homoeopathic Hospital ...	450	4,527	4,977	2,276	2,436	160
Dental Hospital	—	6,530	6,530	1,538	1,731	193
	14,606	255,991	270,597	79,008	96,020	17,012

Among other Birmingham institutions the patients treated were as follows:

	Patients.	Income.	Expenditure.	Deficit.
		£	£	£
Royal Institution for Deaf and Dumb Children	173	8,085	8,869	784
Royal Institution for the Blind	986	9,260	10,612	1,352
Birmingham and Midland Counties Sanatorium	1,757	3,003	2,982	+ 21
Birmingham District Nursing Society	4,918	1,767	2,558	791
Moseley Hall Convalescent Hospital	899	2,219	2,429	210
Jaffray Branch Hospital ...	344	2,240	3,047	807

SOUTH WALES AND MONMOUTHSHIRE.

MEDICAL ATTENDANCE ON MINERS.

THE Executive Council of the South Wales Miners' Federation, which embraces the whole of the South Wales coalfield, had under consideration last week a scheme for the payment of medical men under the National Insurance Act. At the present time in this coalfield a certain poundage is deducted for the medical man by the colliery officials, the amount varying, but being usually 3d. in the £. For this the doctor attends the miner, and his wife and family if he is married. The Act, of course, provides only for the treatment of the insured person, and to meet the new circumstances the following scheme has been drawn up for the approval of the doctors and miners. It is based on the assumption that the medical profession will have come to an agreement with the Chancellor as to the working of the Act.

The scheme recommended is as follows:

1. That where systems or institutions providing medical attendance now exist or their establishment can be brought about, application, in accordance with Regulation 16, should at once be made to the County Insurance Committee for the approval of such system or institution, subject to any modification in its rules necessary to meet the provisions of the Act.

2. That in all other cases the workmen shall make the necessary arrangements, through the medium of the Medical Committee, for the continuance of a weekly payment, either poundage or a fixed payment, for medical attendance on the wives and children, the medical practitioners receiving through such committee the remuneration payable by the Insurance Committee under the Act in respect of the medical attendance on the workmen, and the poundage or fixed payment to be paid, and allowing from the poundage or payment at present paid a sum equivalent in amount to the weekly value of the remuneration so payable by the Insurance Committee.

3. That, as the provisions of the Act in relation to medical benefits of insured persons come into operation on January 15th next, each district of the Miners' Federation shall at once take steps for dealing with the matter referred to in the second recommendation by meeting the medical practitioners concerned with a view to discussing the amount of allowance to be made and of agreeing thereon, and on any other alterations in the present arrangements which may be necessary under the Act, but that, before coming to any final arrangement, the districts shall confer with each other, so as to secure uniformity of policy throughout the coalfield.

The scheme has not been discussed by the profession in South Wales; it will call for very careful consideration. There are some points about it which are not clear. Until the amount of the poundage or fixed payment is settled it will not be possible to form any estimate of the effect these suggested schemes would have on the income of the doctors concerned. In each colliery a large number, sometimes placed as high as 40 per cent., of the men are bachelors; but, under existing arrangements, families, we understand, include sisters who keep house or are dependent upon bachelor brothers, and widows and parents who depend upon the earnings of their single sons or daughters. So that in industrial places single people cannot be regarded as freed from responsibility when they have provided for their own medical care. They may properly be required to pay something towards the family medical fund. The only reasonable means of doing so would be on the lines of insurance, married and single paying alike; so that a single man, by paying a little more than his share when unmarried, would be relieved of the burden that would otherwise fall upon him when he has a family.

BRISTOL.

HONORARY DEGREES.

THE distribution of honorary degrees and installation of the Chancellor, which took place on October 17th, was a well-organized and dignified ceremony. The Colston Hall, where the function took place, was ablaze with scarlet gowns, both of those of the new university degrees and of other universities. The recipients of the widely-distributed honours ranged from the Prime Minister and Mr. Balfour to several whose connexion with the city of Bristol and the university was not very close. Over seventy persons were presented *in corpore* or *in absentia*, so that it is probable that few universities in the world can boast of such a wholesale shower of honours conferred in two hours. It must, however, be admitted that, except for the overpowering noise of the organ, every one was well pleased, and whoever was responsible for the planning and conduct of the pageant is to be heartily congratulated on the manner in which the ceremony was performed. Lord Haldane was resplendent in the black and gold gown of the Chancellor of the University, but beyond a few words of thanks and appreciation of the honour conferred on him, kept his remarks for the address which he delivered in the evening. The medical men honoured by degrees were Mr. Nelson Dobson and Mr. J. Paul Bush, C.M.G., who were made Masters of Surgery. Both of them had been for many years connected with the Bristol Medical School. Dr. R. Shingleton Smith and Mr. Munro Smith were created M.D. for a similar reason. Mr. W. R. Ackland was made a Master in Dental Surgery, having been lecturer on that subject at the Medical School. Dr. J. Michell Clarke and Mr. F. R. Cross were made LL.B., and Dr. Fletcher, of the Index Medicus, an old Bristol student, was made an M.D. Not the least interesting part of the ceremony was to watch the variety of bows the recipients gave on being presented to the Chancellor, but the palm for grace must be accorded—perhaps not unnaturally—to Sir George Alexander.

LONDON.

Treatment of Tuberculosis.

It was reported at the meeting of the London County Council, on October 23rd, that the following letter had been received from the Local Government Board with reference to the provision of accommodation for patients entitled to sanatorium benefit under the National Insurance Act:

Sir,—I am directed by the Local Government Board to inform you that the Government have under consideration the arrangements which should be made in London for the provision of sanatoria and other residential institutions for the treatment of persons suffering from tuberculosis.

The Departmental Committee on Tuberculosis suggested that it should be considered whether some of these institutions should not be provided by the Metropolitan Asylums Board; and the Board have been informed that the Asylums Board would be prepared to undertake the work, and that if authorized to do so they could at once arrange for the reception of a considerable number of cases.

Having regard, however, to the fact that the London Insurance Committee are precluded by the terms of the National Insurance Act from entering into arrangements with the Metropolitan Asylums Board for the reception of insured persons suffering from tuberculosis, legislation would be required before the suggestion of the Departmental Committee could be carried out.

After careful consideration the Government have arrived at the conclusion that it would be impracticable to obtain such legislation in the near future, and the Board, therefore, suggest that the London County Council should act in this matter.

It is desirable that the accommodation which is required should be made available at the earliest possible date, and the County Council will no doubt put themselves in communication with the Metropolitan Asylums Board and other bodies who may have suitable vacant accommodation which could be temporarily utilized without unnecessary expense. As the County Council are no doubt aware, the Metropolitan Borough Councils are taking steps to provide dispensaries.—I am, Sir, your obedient servant,

F. J. Willis, Assistant Secretary.

The letter was referred to the Public Health Committee, which has the whole matter under consideration.

Scotland.

[*FROM OUR SPECIAL CORRESPONDENTS.*]

Medical Conditions in the Highlands.

The Medical Service Committee paid a visit to the west side of the Island of Lewis on October 12th, to take evidence from witnesses representing the parish of Uig. This extensive parish, which is cut up by long arms of the sea, and in parts is very high and mountainous, has only one doctor for 4,500 inhabitants, 700 of whom are resident in the remote island of Bernera. The difficulty of providing medical attendance in such circumstances was very clearly set before the Committee. Interesting evidence was given by Dr. Victor Ross and by Mr. MacDonald, chairman of the parish council. On October 14th another meeting was held in Stornoway to hear witnesses from the parishes of Lochs and Barvas. Geographically, the former presents difficulties as formidable as the parish of Uig, and has a much larger population dependent on one doctor. The parish of Barvas has a population of close on 7,000 living in hamlets scattered along a seaboard of 27 miles, and there is but one doctor. Complicating circumstances, particularly marked in this parish, appeared to be the frequent occurrence of typhoid and typhus fever; in recent years a heavy burden has been thus inflicted on the doctor. All the witnesses testified to the unsatisfactory conditions as to housing and sanitation under which medical treatment had to be carried on, and to the necessity of improvement in these conditions as a preliminary to making any effective medical provision. In some townships infectious diseases had become endemic. It was also pointed out that the doctor rarely gets a holiday, and that this was detrimental not only to him, but to the people he served. An annual holiday would not only make him more fit to stand the strain of Hebridean practice, but it would afford him an opportunity of taking a post-graduate course at a university centre. An interesting fact of vital importance to the inquiry emerged in the course of the evidence. It was that between the Royal Naval Reserve, the militia, the Territorial Force, and the regular army over 4,000 Lewis men were being trained for the defence of the country. The medical witnesses bore testimony to the extraordinary improvement in the health of the people with the coming of spring and summer, and in the case of the children, more particularly with the change to the open-air life of the sheiling. In their case, however, there was evidence of physical deterioration during recent years—due partly to underfeeding, to the use of artificial foods, and to indulgence by the seniors in over-brewed tea. It was also apparent that there is urgent need of educating the mass of the population to realize the value of medical attendance in the early stages of sickness and disease and of systematic care in personal and household hygiene.

The Committee left Stornoway on October 16th, and made a call at Harris, where it heard evidence from the two local practitioners as to the medical wants of that district, and also examined members of local public bodies. At Lochmaddy, later in the day, the evidence of the North Uist and one of the South Uist doctors and the South Uist inspector of poor was taken. Some of the witnesses were in favour of the compulsory levy of a medical rate locally. Mr. Chisholm, Procurator Fiscal for the Long Island, also gave evidence, and expressed the opinion that the present medical service was inadequate, but that the locality was too poor to stand heavier rating than at present. For the Outer Hebrides he favoured a State medical service controlled from some central authority, which would have a voice in the appointments of the medical men, and rigidly supervise their conduct. After a few years' service in the islands they might be promoted according to merit to more desirable posts elsewhere. He thought the central authority should be in London. Mr. Wilson, sub-land commissioner, said the people generally would be able to pay 5s. a family towards medical aid. He had worked such schemes in two parishes, but they could not pay much more. He would approve a State service and a central board directing it, but he would differ from Mr. Chisholm to the extent that he would wish it to be kept in Scotland.

On October 18th the Committee crossed from Lochmaddy to Dunvegan, where the following witnesses from the extensive parish of Durinish were examined: MacLeod of MacLeod, Dunvegan Castle; Dr. Stewart, Dunvegan; Mr. John MacPherson, Glendale; Mr. Kirkpatrick, schoolmaster, Waternish; and the Rev. Mr. MacLean, parish minister of Durinish.

On the way from Dunvegan to Portree a visit was paid to the Geslo Hospital, a free institution for the reception of patients of Skye, provided through a legacy of £30,000 left by the late Mr. Kenneth MacLeod of Greshornish.

Witnesses from the parishes of Portree, Sm̃yort, and Kilmuir were examined at Portree. Evidence was given by Dr. Fletcher, Medical Officer of Health and School Medical Officer; Dr. Stewart, Portree; Dr. MacDonald, Uig, and by several lay witnesses. The club system of payment for medical attendance in the parishes of Durinish and Stoath was discussed. As the doctor could not make a living by fees alone, the parish council had to pay a salary ostensibly for attendance on paupers, but really to provide medical attendance, not only for them, but for poor non-pauper patients. The need of a fuller and better qualified nursing service, of cottage hospitals in the more remote compact townships, and of the development of the telephone system, not only with a view of making the doctors' services more readily available, but especially for communication between doctor and nurse, with a view to her guidance pending, and after, medical treatment, was urged by all the witnesses, as was also the inability of the local rate to meet the necessary cost of making adequate provision for these and other important aspects of the problem. The expenses incidental to public health had in recent years become so heavy a burden on local resources that unless substantial subventions were forthcoming from imperial sources a grave financial situation must be brought about.

Medical Charity in Glasgow.

At Glasgow University, on October 18th, Professor Glaister delivered the opening lecture of a course arranged by the School of Social Study and Training. His subject was A General Survey of the Medical Service of Glasgow, and he gave some remarkable statistics regarding medical charity. The three infirmaries—Royal, Western, and

Victoria—alone expended annually in the charitable medical treatment of the sick poor £108,826, treated 143,657 patients, involving 470,760 individual consultations with the medical staff, and required a staff to handle that vast number of patients of no fewer than 876 doctors, nurses, etc., the medical staff alone numbering 228 doctors, of whom 52 were employed full time and the rest part time. Of the 24 chief institutions in the city, the total annual expenditure amounted to £157,964, the number of patients dealt with to 285,203, involving 637,327 consultations with the medical staffs. The number of nurses was over 1,000, and the number of doctors 450.

In the medical inspection of school children by the School Boards of Glasgow and Govan close upon 39,000 children were inspected during 1911 by a medical staff of 32, of whom 9 were full time and the rest part time officers, and by the aid of a nursing staff of 16.

In the municipal medical service 10,213 patients were medically treated within the wards of Belvidere and Ruchill Fever and Small-pox Hospitals and were nursed and cared for by staffs numbering in all 698 persons. The cost to the ratepayers of these establishments during the year 1911 was £76,392. Of tuberculosis patients within the city 1,348 suffering from lung tubercle were looked after.

Inquiry showed that the membership of the various friendly societies in the city amounted to over 72,000 persons, and had received the services of no fewer than 289 medical men.

Summing up the whole matter, it would not, Professor Glaister said, be overestimating the effect of public medical service of a philanthropic character to say that close on 300,000 persons above the rank of those requiring the services of the Poor Law received medical assistance at Glasgow's medical institutions every year, that over 30,000 received medical assistance from the medical officers of the Poor Law, and that over 76,000 persons had formed themselves into societies to secure medical assistance and monetary aliment during sickness.

THE HOUSING QUESTION.

The housing question is in certain parts of Scotland hardly less pressing than in England and Ireland, and a Royal Commission has now been appointed to inquire into the housing of the industrial population of Scotland, rural and urban (with special reference in the rural districts to the housing of miners and agricultural labourers), and to report what legislative or administrative action is desirable to remedy existing defects. The Commission consists of Sir Henry Ballantyne (chairman), the Right Hon. Lord Lovat, K.C.V.O., C.B., Sir William Younger, Bart., Mr. W. F. Anderson, Mr. G. F. Barbour, Mr. Charles Carlow, Mr. J. F. Duncan, Mr. David Gilmour, Mr. J. Milne Henderson, Dr. W. Leslie MacKenzie, Mr. J. Middleton, and Mrs. George Kerr.

ROYAL MEDICAL SOCIETY AND MEDICAL EDUCATION.

The one hundred and seventy-sixth session of the Royal Medical Society of Edinburgh was opened on October 18th, when Dr. Robert Hutchison, of the London Hospital, delivered an inaugural address on the function of the Royal Medical Society in medical education. He took as his text Bacon's aphorism, "Reading maketh a full man, conference a ready man, and writing an expert man." The society, by means of its library, its debates, and the writing of dissertations, enabled its members to cultivate all three aptitudes. The library was peculiarly rich in old authors, and he expressed the hope that the members would not despise these, because a study of the medical classics gave a proper perspective to the medical outlook, and, what was often lacking in medical writings, a sense of style. Medical current and periodical literature was not so suitable for the student, although, wisely used, it had its use in completing the sense of perspective. He regretted that the society had no department of general literature. General culture was as important as purely professional study. No man was a worse doctor for being a well-read man.

At the close of the address, Mr. R. Simpson, W.S., on behalf of Miss MacFarlan, the last surviving member of the family of Mr. J. F. MacFarlan, founder of the well-known firm of J. F. MacFarlan and Company, chemists, Edinburgh, handed to Dr. Ritchie, one of the presidents, for custody by the society, a very handsome gold medal, which had been presented to Mr. MacFarlan by the society in recognition of his long and valued services as its honorary treasurer.

RECONSTRUCTION OF KILMARNOCK INFIRMARY.

It has been agreed by the directors of the Kilmarnock Infirmary to carry out a scheme for the reconstruction of the building at an estimated cost of £12,000. Dr. Mackintosh, superintendent of the Western Infirmary, and Dr. Burnett, architect, Glasgow, are advising the directorate as to the remodelling of the hospital, and arrangements are being made to obtain the necessary funds by public subscription and otherwise.

THE ANTHROPOLOGICAL MUSEUM, ABERDEEN.

The *Illustrated Catalogue*,[1] by Professor R. W. Reid, of the Anthropological Museum at Aberdeen, with the extra *Catalogue* of specimens deposited by Sir William Macgregor,[2] is of high importance to all who study mankind and analyse prehistoric and ancient art. The catalogues are well got up, cheap, and adorned with good photogravures of flint heads, stone-hammer heads and axes, anvils, and querns, pottery, Egyptian works of art and utility, Asiatic objects of worship, and so forth. It will be of use to the student, with the aid of a manual of anthropology, but it will certainly be yet more prized by experts. The local relics, which include nearly 600 specimens, come first, the museum being rich in Scottish antiquities, from palaeolithic flints to the kayak, paddle, and weapons of a Finn captured in Scottish waters in 1700. Next in order will be found the Wilson Collection of Classical and Eastern Antiquities, put together by Dr. Robert Wilson, H.E.I.C.S., and bequeathed to the College in 1871; the Grant Bey Collection, important to Egyptologists; the Henderson series of early classical vases; and many specimens from other donors, including objects from Africa and the New World. Professor Reid informs us in his preface that the Museum of Marischal College likewise includes a large series of Greek, Roman, Oriental, modern Continental, and British coins contributed by many donors. This series comprises the Davis collection of highly valuable Greek coins acquired by purchase over thirty years ago. The special catalogue of these coins is in course of preparation by the Rev. Professor James Gilroy. Sir William Macgregor has deposited a number of anthropological specimens, mostly derived from the New World. No less than 200 come from British New Guinea, and include remarkable personal adornments, card almanacs, and fishing apparatus.

Ireland.

[FROM OUR SPECIAL CORRESPONDENTS.]

MEDICAL OFFICERS' SALARIES.

THE Belfast Board of Guardians on October 15th considered an important letter from the Belfast Medical Guild, drawing attention to the two following resolutions:

> That in the opinion of the Guild Council no qualified medical practitioner should accept less than £5 5s. per week while acting as locumtenent to any dispensary appointment.
>
> That in the opinion of the Council the minimum salary for a qualified resident medical practitioner should be £130 per annum, with board and rooms giving suitable accommodation for the position he holds.

In support of these resolutions it was pointed out that a private practitioner when engaging a locumtenent would have to pay £4 4s. a week, travelling expenses, and board and lodging; and that a practitioner lately, while acting as locumtenent, paid 410 visits, vaccinated 16 children, attended 3 confinements, for which he received £5 11s., which worked out at 2.92d. per visit alone. A committee appointed to consider the question

[1] *Illustrated Catalogue of the Anthropological Museum, Marischal College, University of Aberdeen.* By R. W. Reid, M.D., F.R.C.S., Professor of Anatomy, Curator of the Museum. Aberdeen: The University Press. 1912. (Demy 8vo, pp. 365, with numerous illustrations. 1s. net.)

[2] *Catalogue of Specimens deposited by Sir William Macgregor, G.C.M.G., M.D.Aberd., etc., in the Anthropological Museum, Marischal College, University of Aberdeen, 1899–1909.* Aberdeen: The University Press. 1912. (Uniform with Illustrated Catalogue, pp. 23.)

of resident medical officers recommended a rotation in the holders of the various posts, instead of the present system of an officer remaining in the one post; and that in future all officers should be appointed on their merits, and that canvassing of the members of the board either directly or indirectly should be a disqualification. The question of salary was deferred.

Dr. Andrew Trimble and the Belfast City Council.

Dr. Trimble has lately intimated his intention of leaving the City Council; he has been a member for about five years, and has proved an energetic and useful member. He was elected Chairman of the Children's Act Committee when it was formed recently; he also had much to do in originating the municipal milk supply, and was influential in bringing about the co-ordinating scheme between the Technical School and the Queen's University. Much regret was expressed at the committees by his fellow members, and he retires with the best wishes and high esteem of all.

The Carlow County Council.

A special meeting of the Carlow County Council was held last week to consider the appointment of a medical officer for the county under the Insurance Act, and to fix a salary. Some time ago the council advertised a post at a salary of £350, and two candidates applied, one of whom, however, was not within the age limit; the other, Dr. Edward McDonald, was appointed, subject to the approval of the Medical Associations. Subsequently Dr. McDonald wrote resigning his appointment, as the Medical Associations would not sanction the salary of £350 a year, and it would not be possible for him to discharge his duties without the co-operation of the other medical men in the county. A letter was read from the Conjoint Committee of the Medical Associations, pointing out that the salary should be at least £400 a year, and further that a sum of less than £100 a year would be inadequate for travelling expenses. A resolution was submitted that the annual salary should be £400 a year, with £50 travelling expenses. The chairman stated that the council was absolutely in the hands of the doctors, and that they could only bow to the superior strength of the medical profession; he would therefore vote for the increase, but his vote was only given under the strongest possible protest.

The county council has decided to send tuberculous patients to the Peamount Sanatorium, paying £70 each for a certain number of beds and £1 a week for each patient while in the hospital. Some opposition was offered to this scheme on the ground that, as it was to be temporary, pending a grouping of counties for sanatorium treatment, the expenditure of £70 each for the purchase of beds should not be authorized.

North Dublin Guardians and Doctors.

At the meeting of the Board of Guardians of the North Dublin Union held last week the following resolution was proposed:

> That, in consequence of the action of the Dublin Medical Committee in refusing medical attendance to members of friendly societies or other such bodies, and excessive demands made by that Committee, and the undue hardships which will operate on the thrifty workers of the city, it is hereby directed that a notice be inserted in the newspapers, and hung up in the various dispensaries, intimating the hours of attendance of the doctors, morning and evening, also the name and address of each guardian and warden from whom tickets may be obtained and the conditions of same.
>
> That the master of the union be directed not to permit any bodies from this union to be sent to the colleges or schools of anatomy, but that in future he sees that all unclaimed bodies are buried.
>
> That all doctors in the employment of the guardians or medical officers who are in receipt of a superannuation be communicated with to ascertain if they are assisting the Dublin Medical Committee in their cruel medical boycott; and that the clerk be directed to have legal opinion as to whether such scheme or superannuations may be cancelled, seeing that the action of the Dublin Medical Committee will necessitate a large number of patients being rejected by the various hospitals, being sent to the workhouse, and the consequent increase in the rates of the city.
>
> That we request the Irish members of Parliament to at once have defined the Medical Act of 1858, and, if necessary, its amendment. Copies of this resolution to be sent to each of the medical officers under this union, and to the members of Parliament representing the county and city.

The chairman of the friendly societies stated that close examination showed that the capitation hitherto paid was 4s. a head, and now the doctors were asking 12s. It should be noted that the 12s. mentioned is the fee for medical attendance on an insured person and family, 8s. being the fee demanded by the doctors for attendance on an insured person. An amendment was proposed to leave out the paragraph referring to the bodies, but this was defeated by 16 votes to 7, and the resolution as originally proposed was carried.

This whole question of disposal of unclaimed bodies by the union was discussed some years ago, when an attempt was made to show that they were sold to the schools of anatomy in Dublin. It was then conclusively proved that the sum paid—about £2—was only sufficient to cover the necessary expenses. Some reference to this "traffic in corpses" was raised at the meeting the other day, but no one pretended that it was the real cause of the refusal to supply the bodies; it was only introduced in order to buttress up their case and enlist sympathy.

Meals for School Children.

For the last two years the Ladies' School Dinner Committee in Dublin has been doing in a small way what in other countries is done by municipalities. It is distributing 450 meals daily in two of the poorest schools in the city. These cost 1d. each for the big children and a little over ½d. each for the smaller ones, and consist of Irish stew on four days in the week, and rice and jam on Fridays. In these schools 15 per cent. of the children come in the morning without having broken their fast. Most of the others have only had quite inadequate breakfasts of bread and tea, often without milk in it. Half the children bring no lunch, and if it were not for the dinners given would remain fasting from 9.30 to 3. Whatever may be the theoretical objection to pauperizing the poor by providing dinners for their children, there is no doubt that it is a waste of time and money trying to teach starving children, and it is probable that this schoolday starvation is largely responsible for a great deal of the tuberculosis in Ireland. It may fairly be argued, therefore, that the feeding of these school children is the lesser of two evils, and that it should be provided for from the rates, as in England, Scotland, and Wales.

Vaccination in Co. Wexford.

Dr. Connolly reported at the last meeting of the Enniscorthy Board of Guardians that there were 266 vaccination defaults in the Oulart dispensary district. This report was marked "read" without discussion, the chairman remarking that they were not going to poison the rising generation.

South Africa.

Regulation of State-aided Hospitals.

An ordinance bearing on this subject has just been passed into law and comes into operation on March 1st, 1913. The medical principles embodied in the ordinance are virtually those laid down by the South African Congress in 1910. The ordinance provides for the constitution of hospital districts, the area of the district being that in which there is a State-aided hospital or dispensary, for each of these districts there shall be a hospital board established, which shall consist of numbers varying from six to thirty-six members according to the size of the district. One-third of the members shall be elected by the contributors at the annual general meeting, one-sixth of the members appointed by the visiting medical officers, and one-sixth of the members appointed by the administrator. This elected board shall have the control and management of every State-aided hospital, dispensary, or convalescent home in the Province at the time of the promulgation of the ordinance. The meetings of the board shall be held once a month and are to be open to the public. Regarding the amount of the Government subsidy which will be paid to every board, it will be on the following scale:

(*a*) Thirty shillings for every pound of the value of all voluntary contributions or gifts of money other than

devises or bequests; (*b*) one pound for every pound of the value of all devises or bequests; and (*c*) one pound for every pound received as fees from patients. The subsidy shall not exceed £500 in respect of the estate of a single testator. If in any year there is a deficit, the administrator shall, if he is satisfied that the board has reasonably endeavoured to collect voluntary contributions, authorize the payment of such deficit in whole or part, as he may deem advisable, in equal proportions, out of moneys appropriated by the Provincial Council for this purpose and out of the funds of the rural or divisional council of the district. The Board has full power to invest any surplus funds, or if need be may borrow money or sell lands held in trust.

Power is given to establish new institutions in a district; they may also, with the previous consent of the administrator, close any institution under its control. Committees of management are to be established, the members being elected by the contributors and the medical officers of the institutions. Rules for the management of the different institutions will be drawn up, but these do not come into force until approved by the administrator.

The administrator shall appoint a medical inspector of hospitals and charitable institutions for the purpose of assisting in the administration of the ordinance. This inspector shall at all times, for the purpose of inspection or the carrying out of his duties under this ordinance, have access to any part of any institution and any books or other records kept by a board or committee thereof.

Correspondence.

TUBERCULOSIS AND THE TEACHING HOSPITALS.

Sir,—Will you allow me a few words relative to the subject of tuberculosis and the teaching hospitals, discussed in a leading article in to-day's Journal?

If, as I trust, the campaign against tuberculosis is to be waged more and more by each medical practitioner in his own sphere, it is essential that he be alive to the great frequency of tuberculous infection, and that he be familiar with refined methods of diagnosis and treatment. Only thus can he be in a position to deal quickly and effectively with the earliest manifestations of tuberculosis—with what has been termed the tuberculous "seedling."

That necessity must be kept in view in the framing of antituberculosis organizations. It was an inspiring element in the creation of what is sometimes known as the Edinburgh system. Indeed, when the tuberculosis dispensary was first projected by me as a practical instrument in handling the great and varied mass of tuberculous disease, I turned towards the teaching bodies and tried to establish the dispensary as a special department of an existing teaching institution.

When the Victoria Dispensary came into being in 1887 as a separate institution, it did so because the opinion of the authorities had not yet grasped the need for special measures in relation to tuberculosis. The proposal to create a scheme of attack on tuberculosis was not received with favour. It was thought unnecessary to put tuberculosis on a different footing from other diseases treated at general hospitals.

It was largely that attitude—natural enough as it was at the time—that actually led to the erection of the tuberculosis dispensary as a separate institution. The incident was not unfortunate, because it permitted the freer development of the ideal of the tuberculosis dispensary as an information bureau in matters pertaining to tuberculosis, a receiving house, a centre of diagnosis, a centre of observation and treatment, a "clearing-house," a centre for the examination of "contacts," and a centre for "after-care."

From the experience of the past there is a good deal to be said in favour of an entirely separate institution which thus centralizes effort in relation to the most frequent and protean of infective diseases. Yet I am far from saying that such separate existence is necessary, provided that, whatever the organic relationships of the institution, the essential purposes and methods of the tuberculosis dispensary are maintained.

Considerations of mere convenience must not be allowed to interfere with the fundamental aim of the tuberculosis dispensary, that not a single case of tuberculosis shall remain undetected and uncared for, and that not a single dwelling in which tuberculosis has occurred shall escape attention.

In teaching centres it should not be difficult to link the tuberculosis dispensary and other institutions devoted to the prevention and treatment of tuberculosis with the teaching school. In the case of Edinburgh, the tuberculosis dispensary has been used ever since its foundation as a training ground for students and graduates. Since 1889 regular courses of instruction in the diagnosis and treatment of tuberculosis have been carried on in connexion with the school of medicine by members of the dispensary staff. The attendance of medical practitioners at the ordinary visiting hours has been welcomed both at the dispensary and at the hospital.

Co-ordination of effort, which has throughout been the *motif* of the Edinburgh system, includes not only the harmonious co-operation of the several elements in the antituberculous scheme, but also the linking of these elements with other agencies connected with the problem, including the medical school.

Numerous facts point to the conclusion that success in the eradication of tuberculosis will depend especially on anticipation and prevention of its occurrence in childhood. In any case it is clear that the secret of success lies in the earliest possible determination of the presence of disease, and this in turn is dependent on the training of men who shall be alive to the possibilities, quick to perceive, and ready to act.—I am, etc.,

Edinburgh, Oct 19th. R. W. Philip.

TUBERCULIN TREATMENT.

Sir,—In discussing the advantages of tuberculin dispensary treatment, as compared with sanatorium treatment, I do not think it is always clearly explained that everything that is done at a dispensary can be done better at a sanatorium. When discussing the cost of the two treatments, it is left out of account that many cases going to dispensaries do not require treatment in that particular way, but might well be treated by their own doctors. If the figures available from districts such as Greater New York (where compulsory notification has been enforced for a number of years) are examined, it will be found that the number of notifications for any year exceed by three or four times the number of deaths notified from pulmonary tuberculosis. We may infer that 60 or 70 per cent. of those notified recover; if, therefore, the net be drawn wide enough, we cannot help securing good results from any treatment that is not in itself harmful. The number of deaths occurring from pulmonary tuberculosis in any given year, in so far as it is accurate, will indicate the number of cases where treatment has failed.

Because a chronic case—the chronicity often being due to neglect of proper treatment in the early stage—can be treated with some measure of success by a given method, it does not follow that an early case should be treated in the same way, nor does it follow that because people desire to follow their employment whilst being treated that they should be encouraged to do so. Such a course results either in the production of a chronic or of acute disease, both of which conditions may be very difficult to cure; such cases provide the majority of bacilli carriers. The use of diagnostic injections of tuberculin is open to grave suspicion. If the diagnosis be so difficult it would seem to follow that the patients are not so very ill, but if such a dose of old tuberculin be administered to produce a focal reaction in the lung a typical case of early phthisis with physical signs is produced, and, although the immediate ill results may soon pass off, a lesion may have been produced which it may or may not be possible to check, and at the same time a certain amount of toxin tolerance which may mask any symptoms of activity produced by the injection; larger doses of tuberculin will probably have to be used than would have been necessary had a very small dose, incapable of producing a focal reaction, been used at first. If diagnosis be made from symptoms and from correct records of temperature, the patient being put to bed, there

would seem to be very few cases where a diagnostic injection could be regarded as justifiable. It would seem that resting a patient in bed increases the sensitiveness to the benefits of tuberculin, whereas injecting tuberculin decreases this sensibility.

It would appear that of 324 patients who received diagnostic injections of tuberculin at Tübingen Medical Hospital between October, 1904, and October, 1909, 197, or 60.8 per cent., gave a focal reaction, together with a general reaction, 24, or 7.4 per cent., focal reaction only; 76, or 23.5 per cent., of a general reaction only; 27, or 8.3 per cent., neither focal nor general reaction. Of the 18 showing no reaction and receiving no treatment 17 remained healthy, whilst one had developed fistula in ano, and laryngeal tuberculosis. Out of 47 examined later, who had given only a general reaction, 44 were healthy, 3 showed deterioration in lungs and general condition. Subsequently, of 135 who had given focal reaction, 2 had died, 82 were worse, and in 51 only was permanent arrest obtained and restoration to working capacity.[1] The above is well open to the criticism that those patients who exhibited a focal reaction did worse than had they been left alone, for presumably, unless the injections were given from mere wantonness, these patients could not have been showing marked symptoms. It seems probable that a large number of these patients might have been a long while, perhaps years, before such a disturbance of their economy, as that produced by the tuberculin, would have occurred.

I agree with Dr. Hyslop Thomson that physical signs should improve, when patients are receiving tuberculin. If they do not, then some other form of treatment should be adopted at once.—I am, etc.,

Ayrshire, Oct. 21st. EDWARD E. PREST.

SIR,—The letter by Dr. Wilkinson (p. 1080) is a straightforward statement of facts proved by himself in practical everyday work. Any one who has taken the trouble to attend his dispensary in Kennington Road, London, must be convinced of the scientific truthfulness and accuracy of his statement. My stay at his dispensary, though short, was quite sufficient to convince me, and my subsequent work with his methods has given me and my patients every satisfaction. All my work is done in private practice, having no hospital appointment. This fact goes a long way to prove one of his assertions—that is, that his method of giving tuberculin is the most suitable one for general practitioners.

The two letters by Drs. Thomson, Harris, and Coppock are a mixture of theory and destructive criticism, and so call for no further comment than to say that Dr. Coppock might have hit on a happier way of expressing himself *re* "disciples of Dr. Camac Wilkinson." I am one of those "disciples," and my being so depends on personal observation and practical experience; therefore I resent very much Dr. Coppock's words.

I hold no brief for Dr. Wilkinson, nor does he need support from me, but it gives me great pleasure to acknowledge his courtesy to me in allowing me to examine his patients and learn something of his methods. I have found his work good, and I believe it will endure.—I am, etc.,

Lisburn, Oct. 19th. J. L. RENTOUL, M.B.Edin.

THE ORGANIZATION OF THE CAMPAIGN AGAINST TUBERCULOSIS.

SIR,—You publish this week a long letter from a correspondent purporting to be an answer to an attack supposed to have been made by me in a communication which appeared in your issue of September 28th. Any one who read the communication will see that no such attack was made. I was merely concerned to point out the danger to medical science of stereotyping by official regulations any methods of treating tuberculosis regardless altogether of their special nature or supposed merits. Your correspondent and any particular methods he may claim to employ were not referred to. They were not in my mind. If I should ever think fit to criticize them, your correspondent will be in order in trying to disprove my competence as a critic or in taking any steps he likes to endeavour to silence me. Until then your correspondent's assumptions as to my "bias against" or "unshaken belief" in any form of treatment are without evidence and, in the parliamentary sense of the word, impertinent.—I am, etc.,

London, W., Oct. 22nd. LAURISTON E. SHAW.

TREATMENT OF NEURASTHENIA.

SIR,—I have read with much interest the article by Dr. Brook in the JOURNAL of October 12th, p. 964. For the last six months I have been treating patients with similar symptoms by a vaccine of a diploid streptococcus and a staphyloid coccus urinae, having previously in each case had the diagnosis confirmed by detection of these organisms in the urine. I have been doing this on the suggestion and under the direction of Dr. Warren Crowe of Yelverton, who for some time has been doing valuable pioneer work on the subject.

Within the first month the anaemia, lassitude, "rheumatism," frequency of micturition, irritability, and insomnia have been ameliorated with a decisiveness and rapidity that have astonished me. I have never known a haematinic so rapid in effect. The second and third months of treatment have confirmed the result. The question is, Would not a microscopic and cultural examination of the urine, without serological observations, have revealed as much in these cases of Dr. Brook's, and given the same clue to treatment? As a country practitioner I aver that these symptoms are to be met with as frequently among country folks as among the dwellers in cities.

We have been recently told by an advocate of suggestive medicine that 50 per cent. of the cases a practitioner treats are functional. It would appear that some of these functional disturbances have for efficient cause a coccal or coliform invasion from a mucous tract, of saprophyte turned pathogenic under conditions of lowered vitality the result of faulty hygiene or infection from without. They are at least not all neuropaths by inheritance and perhaps we need hardly yet become followers of Mesmer.—I am, etc.,

Liskeard, Oct. 15th. JOHN DONALD, M.D.

THE INDIAN MEDICAL SERVICE.

SIR,—Your correspondent, "Capt. I.M.S.," whose letter appeared in the JOURNAL of September 28th, suggests that my letter published in the issue of September 21st is wanting in *esprit de corps*, and states that he is unable to follow my motives.

I would ask him, Is it not truer *esprit de corps* to look facts in the face, and strive for better things for one's service than to shut one's eyes to deficiencies and to declare them, therefore, non-existent?

The I.M.S. was a fine, a very fine, service, but can that be honestly said of it now? The truth is "Capt. I.M.S." and I look at the question from a different standpoint; he views them as a young, probably unmarried, officer in civil duty, and I as a more senior married man in military employ. In the past the military side of the I.M.S. has undoubtedly suffered and been neglected, the bait held out of possible civil employment being sufficient to attract candidates to fill vacancies in the service. Remembering that the I.M.S. is primarily a military service, such a state of affairs is obviously very unsatisfactory.

No doubt there are even yet some attractions on the civil side of the service. For instance, furlough pay, as mentioned by "Capt. I.M.S.," he now draws £500 as against the £300 per annum he would get if in military. (I think he might have added that £500 per annum is civil furlough pay irrespective of the amount of an officer's service—that is, "Capt. I.M.S." would draw the same furlough pay in, say, ten years' hence as he does now.) Then, again, the young officer in civil evades the discipline of mess and military duty generally. I fancy your correspondent's motive for writing was probably that he was restive under the idea he might have to return to military duty if my supposition of the future of the I.M.S. is correct.

If "Capt. I.M.S." will observe the signs of the times, he will see that there is a very grave and justly founded feeling of discontent in the service—a feeling of being "done." During the past few weeks no less than four majors have resigned the service (see JOURNAL). If the service is attractive, why should comparative junior men be so

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[1] *British Journal of Tuberculosis*, October, 1912, pp. 207 and 208. (Barcroft.)

anxious to leave it directly they are qualified for early pension, instead of waiting for higher pension?—I am, etc.,

October 7th. MAJOR I.M.S.

DIPLOMAS AND DEGREES.

SIR,—I had the pleasure of being present at the very interesting address given by Dr. Seymour Taylor at the West London Post-graduate College at the opening of this session on October 14th. Medical authorities of the day seem to consider it an undisputed fact that a man with any university qualification must be superior to one possessing that of the Royal Colleges, and it is on this point that I wish to call the attention of your numerous readers. Again, it is taken for granted that one with only such letters as L.S.A. London or Dublin to his name cannot but possess a very superficial knowledge of his profession, and here again I come forward to protest against such views.

It must be remembered that many a student who seems to possess no higher ambition than the L.S.A., and studies for such, does so not on account of any lack of intelligence on his part, or even to want of confidence in his ability, but may be due to mere force of circumstances.

Dr. Beddard, now Senior Physician, West London Hospital, after proposing a vote of thanks to Dr. Taylor, very correctly remarked, in expressing his views of medical education of to-day, that students studying for the L.S.A., Royal Colleges or any university, pass through much the same courses of study. Why, then, should the lower qualifications be despised when often it is only a matter of one man having a more severe test than another to obtain for him an entrance into the noblest profession existing?

As long as England has these various grades of qualifications it must be expected that students will choose for themselves, according to their means or other circumstances. It is by no means uncommon to find that a man once he holds the coveted letters of M.B., B.S., considers he has all he wants, and thus leaves studies aside for a shorter or longer period, and perhaps sometimes for good. On the other hand, men with only the Royal College or Hall diplomas, realizing their disadvantages, strive hard by constant study to be considered equal to their brother M.B., or to do justice to the post that they are holding, with often more than the desired result.

The proof of the pudding is in the eating. Take the results of the examinations into any of the services—for example, the Indian Medical Service, one of the best all-round tests. Men who are licentiates of the Royal Colleges do every bit as well as those holding university degrees, and not infrequently one with only his modest L.S.A. passes in, leaving behind him many a Royal College or university man. The greatest drawback in the Colonial Service is the fact that no competitive examination is held for entrance, and so with only qualifications to be guided by, many a good hard-working applicant is rejected, unless backed by influential superiors.

I suggest that the posts of house-physician and house-surgeon should be made by examination. It is only fair to give men equal chances of soaring to a lofty niche in the Temple of Fame. Some hospitals do carry out this plan—a perfectly fair and just procedure—but there are still many institutions that would do well to adopt the same method.—I am, etc.,

October 16th. HALL AND ROYAL COLLEGES.

WHOOPING-COUGH AND IODOFORM INJECTIONS.

SIR,—May I indicate to Dr. Bain that he has quite missed the point of my letter of September 28th; also that he has begged the question at issue no less than four times in his short letter? It is just from that assumption of cure by the specific means employed, so easily reached by Dr. Dewar, so readily accepted by Dr. Bain, that I dissent.

I will only add that, in his selection of a case to support Dr. Dewar's example, Dr. Bain is extremely unhappy, and that it also illumines his personal equation as embracing therapeutic optimism of a marked degree.—I am, etc.,

Glasgow, Oct. 10th. R. O. ADAMSON.

Universities and Colleges.

UNIVERSITY OF OXFORD.

THE following degrees have been conferred:

M.S.—H. S. Souttar.
B.M., B.S.—N. S. Lucas.

UNIVERSITY OF CAMBRIDGE.

THE following candidates have been approved at the examination indicated:

D.P.H. (BOTH PARTS).—A. B. Arora, *A. T. M. Blair, W. Brown, J. C. G. Carmichael, Capt. R.A.M.C., A. J. Clark, T. G. D. Cooper, J. T. Dick, G. Fletcher, S. N. Galbraith, C. M. Ganapathy, T. S. Gibson, L. C. Hayes, Lieut. R.A.M.C., C. B. Heald, T. J. Hughes, J. McGilchrist, G. Macleod, R. K. Mhatré, H. W. Moir, J. McI. Morgan, J. Selfe, C. H. S. Taylor, G. Walker, A. White.

* Passed with distinction.

Degrees.

The following degrees have been conferred:

M.D.—G. Graham, A. B. Howitt.
M.C.—F. J. Cleminson.
M.B.—H. Bowring, A. C. Gemmell, C. B. Wainwright, L. N. Weeks.
B.C.—H. Bowring, J. Ellison, A. C. Gemmell, C. E. Redman.

UNIVERSITY OF LONDON.

KING'S COLLEGE.

Special Lectures in Physiology.

THE course of four lectures on the mammalian heart to be given by Dr. F. S. Locke in the Physiological Laboratory, King's College, on Mondays, November 11th, 18th, 25th, and December 2nd, at 4.30 p.m., are free to all medical students in London medical schools, to internal students of the university, and to medical men on presentation of their cards. The following is the syllabus of the lectures: "Recent Methods of Investigation of the Mammalian Heart. The Isolated Heart. Graphic Methods. Electrical Methods. Automaticity, Rhythm, and Excitability of the Heart. The Nutrition of the Heart. The Innervation of the Heart and of the Coronary Circulation. Heart Failure. Some Points in the Mechanics of the Heart and of the Coronary Circulation."

KING'S COLLEGE HOSPITAL MEDICAL SCHOOL.

The following entrance scholarships have been awarded at the medical school of King's College Hospital: *Anatomy and Physiology*, value £50, Mr. H. Rivers Pollock (Trinity College, Cambridge); *General and Clinical Pathology and Pharmacology*, value £50, Mr. E. Watson Williams (Caius College, Cambridge). The Epsom Scholar is Mr. R. H. Leigh.

ROYAL COLLEGE OF PHYSICIANS OF IRELAND.

AT the annual stated meeting of the President and Fellows of the Royal College of Physicians of Ireland, held on October 18th (St. Luke's Day), the following officers were elected for the coming year:

President.—Dr. C. E. FitzGerald.
Vice-President.—Dr. James Craig.
Censors.—Dr. James Craig, Dr. T. G. Moorhead, Dr. A. Nixon Montgomery, and Dr. T. Henry Wilson.
Examiners for the Licence to Practise Midwifery.—Dr. G. FitzGibbon and Dr. Holmes.
Additional Examiners to take the Place of an Absent Censor or Examiner.—Medicine: Dr. Parsons. Medical Jurisprudence and Hygiene: Dr. Bewley. Midwifery: Dr. Hastings Tweedy.
Supplemental Examiners under the Conjoint Scheme.—Biology: Dr. E. MacDowel Cosgrave. Chemistry: Dr. E. Lapper and Dr. N. Falkiner. Physics: Dr. E. J. Watson and Dr. W. G. Harvey. Pharmacy, Materia Medica, and Therapeutics: Dr. Travers Smith and Dr. D. J. O'Connor. Physiology: Dr. H. C. Earl. Pathology: Dr. F. C. Purser. Medicine: Dr. J. F. O'Carroll and Dr. H. C. Drury. Hygiene and Forensic Medicine: Dr. W. A. Winter.
Examiners for the Conjoint Diploma in Public Health.—Chemistry: Dr. E. Lapper. Hygiene: Dr. H. T. Bewley. Bacteriology: Dr. H. C. Earl. Meteorology: Dr. W. G. Harvey.
Examiners for the Membership of the College.—Practice of Medicine: Dr. Martin Dempsey and Dr. T. G. Moorhead. Clinical Medicine: Dr. J. Craig and Dr. George Peacocke. Pathology: Dr. H. C. Earl and Dr. O'Sullivan. Obstetrics and Gynaecology: Sir William J. Smyly and Dr. A. J. Horne.
Representatives of the College on the Committee of Management.—Dr. Walter G. Smith, Sir John W. Moore and Dr. T. P. C. Kirkpatrick.
Representatives of the College on the General Medical Council.—Sir John W. Moore.
Treasurer.—Dr. H. T. Bewley.
Registrar.—Dr. T. Percy C. Kirkpatrick.
Librarian.—Mr. R. G. J. Phelps.

ROYAL COLLEGE OF SURGEONS OF EDINBURGH.

THE following candidates have been admitted to the Fellowship:

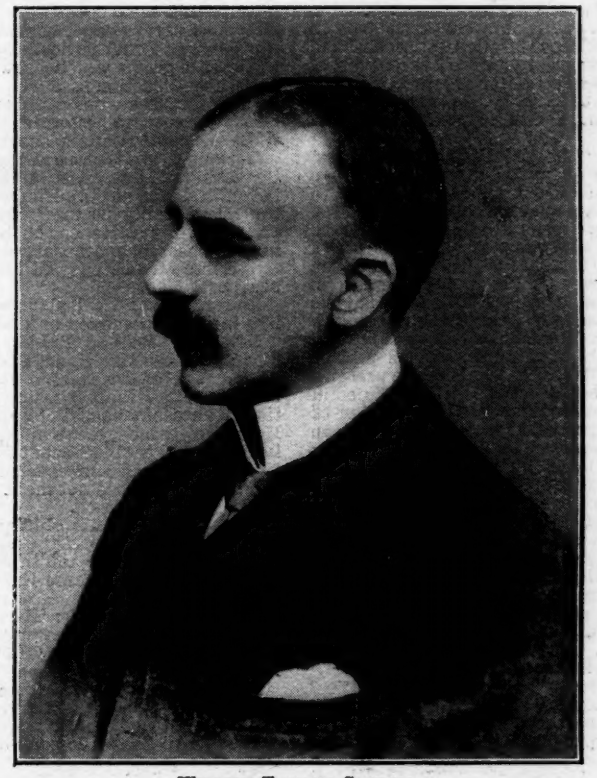
G. W. Bury, R. G. M. Clements, J. G. Cormack, A. Fells, H. W. Gabe, N. J. Gerrard, A. C. Hendrick, C. Hudson, D.S.O., Major, I.M.S., J. W. Illius, Captain, I.M.S., F. Kahlenberg, V. Lloyd-Evans, R. B. Macfie, A. McMurray, L. Myer, C. T. H. Newton, G. F. Porter, W. Ross, W. E. Scott-Moncrieff, Major, I.M.S., A. A. Straton, L. W. O. Taylor, W. S. Thacker.

Obituary.

WILLIAM TUSTING COCKING, M.D.,

HONORARY PHYSICIAN TO THE SHEFFIELD ROYAL INFIRMARY; EMERITUS PROFESSOR OF PHARMACOLOGY AND THERAPEUTICS, SHEFFIELD UNIVERSITY.

It is with profound regret we record the death of Dr. William Tusting Cocking, on October 17th, after a long and distressing illness which he endured with rare fortitude. The cause of his death was pulmonary tuberculosis, and there can hardly be a doubt that he contracted the disease in the out-patient department of the Sheffield Royal Infirmary, where for many years he made a special study of phthisis.



William Tusting Cocking.

Photograph by [*R. S. Henderson, Sheffield.*

He was the son of the late Mr. Tusting Johnson Cocking, of Sheffield, and was born in 1862. He was educated at Wesley College, Sheffield, and at University College, London, where he greatly distinguished himself; he became M.R.C.S. in 1884, and in 1885 took the degree of M.B.Lond. with honours in medicine and obstetric medicine. Two years later he proceeded to the M.D. Lond., qualifying for the gold medal. After holding the usual resident appointments at University College Hospital he returned to his native city, and was appointed assistant physician to the Sheffield Royal Infirmary, and thus began a connexion with that institution which ended only with his death. In due course he was promoted to be full physician to the Royal Infirmary, and for upwards of twenty years he worked incessantly for the sick poor of Sheffield, earning the gratitude of a multitude of patients.

He took a very large share in the management of and the teaching in the medical faculty of the Sheffield University. He was the first Dean of the Faculty, and for many years held the Chair of Pharmacology and Therapeutics. On resigning the latter, owing to his illness, he was appointed Emeritus Professor. His clinical teaching in the wards of the Royal Infirmary attracted at all times a large class of students, with whom he was always on excellent terms.

He was the first representative of the university on the General Medical Council, and held this office as long as the state of his health permitted.

For several years he acted as Secretary to the Sheffield Medico-Chirurgical Society, resigning this post on his election to be President in 1906. He was a member of the British Medical Association, and was Secretary of the Section of Pharmacology and Therapeutics at the Annual Meeting at Portsmouth, 1899. When the Association met in Sheffield in 1908 his health was already too precarious to justify him in accepting any office.

He took an active part in the editing of the *Quarterly Medical Journal*, and contributed many articles to it.

A good observer, a man of keen insight and sound judgement, thoroughly interested in his work, and doing with all his might whatever he undertook, he made an ideal hospital physician. Singularly modest, perhaps even to a fault, and self-sacrificing to an unusual degree, he delighted to advance the interests of others rather than his own. His reward was the universal esteem of all with whom he came in contact. While tolerant of the weaknesses of the weak, he could, when the necessity arose, be a severe critic of those in high places, but his keen wit was reserved for the few who were privileged to belong to his inner circle of friends. The views he held of mankind are well summarized in R. L. Stevenson's aphorism: "There is so much bad in the best of us, and so much good in the worst of us, that it ill behoves any of us to find fault with the rest of us." Dr. Cocking married Miss Alice Mary Birks, the daughter of the late Mr. Edward Birks, of Sheffield, who with one son and daughter survive him. He was buried in Fulwood Churchyard on October 21st.

Thomas McClure, M.D.Edin., F.R.C.S.I., who died at Luton on October 1st, aged 65 years, entered the medical profession in 1868, and joined the Bath and Bristol Branch of the British Medical Association early in the Seventies. He spent forty-four years in hard general practice, mostly in the country, and wherever he practised he soon attracted by his kind, sympathetic, and genial manner a large number of patients. He was at times misunderstood by his professional brethren, but those who were best acquainted with him knew how, with a real heroism for the sake of those around him, he bore silently and with an extraordinary cheerfulness strokes of misfortune, and, during his last months, the agony and distress of a disease that he must from the first have known to be fatal. He leaves a widow and eight children. Two of his sons joined the profession; the oldest, Charles, died of enteric fever at Koffyfontein, South Africa, only five weeks before his father. The other, Walter, is now assistant medical officer of health for Manchester.

We had with regret to announce last week the death of Mr. John Marle, who had been attached since 1865 to the Museum of the Royal College of Surgeons of England. Working as assistant articulator under Mr. James Flower, he succeeded to the senior appointment in 1876. He was well known to anatomists as an expert in identifying fossil bones, and his merits were recognized and appreciated by the three conservators under whom he worked —Sir William Flower, Professor G. Stewart, and Professor Keith. He was yet more widely known to a whole generation of students as an assistant at the College examinations, his voice as he ushered in the candidates being very familiar to the Court. He died in University College Hospital of pneumonia and empyema, at the age of 67.

Deaths in the Profession Abroad.—Among the members of the medical profession in foreign countries who have recently died are: Professor Otto Soltmann, director of the Clinic of Children's Diseases of the University of Leipzig, aged 67; Dr. Danlos, physician to the Hôpital Saint Louis, Paris, and a recognized authority on diseases of the skin and syphilis, aged 68; Dr. Joseph Schmitt, professor of clinical medicine in the University of Nancy, and author of works on microbes and disease, the diagnosis and treatment of infectious diseases, and other contributions to medical literature, aged 57; and Professor Hermann Munk, the famous physiologist of Berlin, aged 73.

Medico-Legal.

STEVENS v. BRITISH MEDICAL ASSOCIATION.

THIS case, the hearing of which was commenced on October 22nd before Mr. Justice Pickford and a special jury, was an action in which Mr. Stevens, managing director of Stevens & Co., Ltd., formed for the sale of a specific for the cure of consumption, sued the British Medical Association for damages sustained through statements published by the Association in *Secret Remedies, What they Cost and What they Contain*. The defendants denied that the words published were capable of the meaning alleged, and said that so far as the allegations consisted of fact, they were true, and so far as they were expressions of opinion, they were fair comment on a matter of public interest.

Mr. Tindal Atkinson, K.C., and Mr. H. W. Rowsell appeared for the plaintiff; and Mr. Holman Gregory, K.C., and Mr. Colam, K.C., for the defendants.

Mr. Tindal Atkinson, in opening the plaintiff's case, said that the statements in the book were founded on analyses, and on the face of these the defendants had charged Mr. Stevens with being a swindler and a quack, and a quack who foisted upon the public a remedy which he knew to be absolutely worthless. The defendants relied on their analysis and on the names of the plants the plaintiff said he had used not being found in the British or South African *Pharmacopoeia*. He quoted the analysis as follows: "The medicine was a clear red liquid, and analysis showed it to contain in 100 fluid parts 21.3 fluid parts of alcohol, 1.8 parts of glycerine, and 4 parts of solid substance; this solid substance contained about 1 part of a tannin and 0.2 part of ash; the remainder being extractive. No alkaloid was present, and no other active substance could be detected. The solid substance agreed in all respects with the solids of decoction of krameria, or a mixture of this decoction with a little tincture of kino. The formula thus appears to be approximately: Rectified spirit of wine, 23.7 parts by measure; glycerine 1.8 parts; decoction of krameria (1 in 3) to 100 parts by measure, or it may be made with tincture of krameria."

Mr. Tindal Atkinson said that if that analysis were true there was a fraud on the part of the plaintiff. He would be making his remedy from krameria and kino, costing a few pence, and selling drugs which were useless for the purposes for which the remedy was sold—and useless to his knowledge—at a greatly increased price. If that were so, there was an end of Mr. Stevens's case and of his reputation. But if the defendants failed to establish their analysis, then the way was open to Mr. Stevens, who, he submitted, would be entitled to their verdict.

Continuing, Mr. Tindal Atkinson said that the plaintiff was born in Birmingham in 1880, and in 1897, as he was out of health and losing flesh, consulted his doctor, who told him that he was suffering from tuberculosis of the lungs. This opinion was confirmed by a specialist and the plaintiff was advised to go to South Africa. Accordingly in 1897 he went to Bloemfontein, and there learnt of an Englishman suffering from consumption who had been treated by a native and after three months had been able to return to England perfectly cured. The plaintiff submitted himself to the treatment of this native, and was supplied with a substance which he was told to boil, and to drink the decoction. At the end of two months he lost practically all symptoms of consumption and returned to England, when his doctor reported that he was cured. Subsequently he served with the Scottish Yeomanry in the Boer war, and afterwards in the Cape Mounted Police. He started in business as a motor and cycle agent in Cape Town, but owing to the destruction of uninsured property by fire the business came to an end. The plaintiff commenced to advertise the remedy under the name of Saccom, and endeavoured to form a company for its sale under the name of Sacco, Limited. In this he did not succeed. He had added to the substance a plant called chijitse, a name which was an attempt to express a well-known clicking sound used by the Zulus. The object of this substance was to stop bleeding. Mr. Stevens had cured himself and had treated hundreds of people with the remedy with almost invariable success. Amongst the cures were those of people who had been discharged from sanatoriums and hospitals as incurable, but had yet been cured. Counsel intended to call a number of them as witnesses, and also a number of doctors who had watched the cases, and the jury would be asked to award Mr. Stevens substantial damages.

Mr. Edwards Morrell Holmes, examined by Mr. H. W. Rowsell, said that he had been curator of the Pharmaceutical Society of Great Britain since 1872, had been lecturer on materia medica from 1887 to 1890 for the Society, and had been president of the British Pharmaceutical Society in 1900. The plaintiff submitted to him in December last two substances, a plant and a herb. He recognized that the substance called chijitse belonged to the natural order of *Geraniaceae* and grew in South Africa; it was not recorded as growing in any other country. It was impossible to say where the root called umckaloabo grew. From a microscopical examination he would say that it belonged to the *Polygonaceae* family, which was found all over the world. Krameria and umckaloabo were distinct in physical characteristics, and on microscopical examination; kino was not a root but an extract from the bark of an Indian tree. Neither krameria nor kino came from South Africa.

Cross-examined by Mr. Holman Gregory, witness said that when the plaintiff brought him the roots he told him they came from South Africa. He knew the botanical name of chijitse, but did not know whether he ought to mention it.

Mr. Justice Pickford: You mean it is the plaintiff's secret; then write it down.

The witness wrote on a piece of paper and handed it to counsel.

Mr. Holman Gregory: That is the botanical name; has it got a popular name among the natives at South Africa?—Yes, it is spelt in various ways—nwta, geita, and keita. Hottentots and other natives use it for colds and inflammation; also for anthrax, dysentery, and chronic diarrhoea; also for convulsions and cough and chest affections—as a demulsive. It appears to have been known locally for 100 years. As to the substance known as umckaloabo, he was not in position to say whether it ever grew in South Africa. He could only say it was a root of a plant of a group which grew all over the world. He had not made any decoction from it.

The court adjourned at this point.

The cross-examination of Mr. E. M. Holmes was continued when the court resumed on October 23rd.

In reply to Mr. Holman Gregory, witness said that there were three kinds of krameria, but no species of krameria was known in South Africa at all. Rhubarb, dock, and sorrel were roots of the same family as umckaloabo. They possessed astringent and other properties, one of their properties being to stop bleeding. Monsonia, another name for chijitse, had been mentioned in the *Lancet* as being good for dysentery, and it might possibly be a germicide.

Re-examined: He had never come across anything exactly like the root submitted to him.

Mr. Otto Hehner, Public Analyst for Nottingham, West Sussex, Derby, and Ryde, examined by Mr. Rowsell, said that he had analysed a sample supplied by the plaintiff. It contained neither krameria nor kino. He had prepared a decoction of umckaloabo and chijitse. The reactions of both agreed. He had tested Stevens's mixture in many different ways, and in his view it could not contain krameria in any tangible quantity. He was also of opinion that there was no kino in the mixture, but he had not made so many tests with the object of finding that substance.

Cross-examined: He obtained the sample of Stevens's mixture in March, 1912. If the analysis on p. 32 of *Secret Remedies* was correct the two mixtures were entirely different. If the mixtures were the same the analysis of the defendants was inaccurate.

Mr. Holman Gregory: The defendants' analysis was made on June 17th, 1908. Continuing, witness said that in a decoction of krameria the solids would vary. There would, however, be no variation in the reactions.

Re-examined: Quantitative and qualitative analysis were entirely distinct, but even by qualitative analysis he was satisfied that the two mixtures were different.

Mr. John Percival Lord said he was an analyst and bacteriologist living at Wimbledon. He had had twenty-two years' experience. In his view the plaintiff's medicine was not made of krameria, nor did it contain any; krameria and Stevens's mixture differed in specific gravity, total solids, etc. He had analysed Stevens's mixture on many occasions since 1908, and was quite satisfied that it contained no krameria. Although the quantitative results varied slightly, the qualitative results were always the same.

Cross-examined by Mr. Holman Gregory: He knew the plaintiff, but had never lived with him at Wimbledon, although he had worked in his office. He had worked in a special laboratory which the plaintiff had fitted up for him. The plaintiff's mixture was advertised considerably, and eight or ten young ladies were employed taking down letters for dispatch. He had been employed by the plaintiff from January to March, 1911, and again from December 21st, 1911, to June, 1912, being paid a guinea and a-half a week. In 1906, 1907, and 1908, he had been bacteriologist for the Councils of New Malden and Leatherhead. In that at New Malden he had a small annual retaining fee, but otherwise he was paid by fees. He only saw a part of the correspondence which was sent out from Mr. Stevens's place, as his work was on the top of the building. There was one room in which packing and dispatching was done, and another for the correspondence.

In re-examination, he said that prior to 1911 he was not employed by the plaintiff, except with regard to work on a sample a doctor had sent him.

Mr. Thomas Dalkin, of the South London Laboratories, said that he was a qualified pharmacist. Although not now a member of the Pharmaceutical Society, he was still on the roll. He had no connexion of any kind with the plaintiff. He had found no krameria or kino in the plaintiff's samples, and did not know of the existence of Stevens's Consumptive Cure until he received the samples.

In cross-examination, he said that he earned a salary of £2 10s. a week and commission at the South London Laboratory conducted by Mr. Muter. He had been allowed to do the work, as the firm was busy at the time. He had received typewritten instructions with the samples, but could not now produce them. His object was to find out whether the samples contained krameria or kino. His experiments occupied his evenings for a month.

Mr. Edward Ferdinand Grün, L.R.C.P., M.R.C.S., a medical officer of the Steyning Union, in giving evidence, said that he was in practice near Brighton. He had had experience of tuberculosis, and had written a book on Dr. Koch's treatment of that disease. He was then at the Hospital for Incurables at Putney. He was also the author of a number of letters to the *Lancet*.

In 1908 he received a circular relating to Stevens's Consumption Cure. He then wrote to Stevens, who sent him further papers, pointing out the very striking results obtained. During the thirty years of practice he had lost many dear friends from tuberculosis, and in 1908 one had just died, and he accordingly felt it his duty to try every remedy. With the consent of some patients he gave them Stevens's mixture, and was very much satisfied with the beneficial results. One patient, a woman named Hudson, who asked for relief was in a very bad condition, having a temperature of 103°. She could not eat. Upon making use of the mixture her condition improved, and at the end of fourteen days before the next meeting of the guardians her husband was able to send her away. Another patient, a lad at Shoreditch Grammar School, had a cavity at the apex of the lung, and became so bad that he could not go to school. After three months' treatment with Mr. Stevens's mixture there was no mischief in the lung and no tubercle. There was no relapse. Another patient whom he had treated successfully had come to court to-day. He had been certified by four doctors to be suffering from tuberculosis. In 1908 he had had a bad attack of the disease, but under the plaintiff's treatment he had put on weight. He had been sailing a yacht round the coast all this wet summer. A gardener in the witness's employment, named Pusey, a man of good physique, weighing 11 or 12 st., had suddenly been taken ill with galloping phthisis. In a month he weighed only 7 st. When witness returned from abroad he found this man very bad, but on treating him with the mixture he improved. He was subsequently attacked by influenza and died of pneumonia. On the whole he thought the treatment very beneficial, and he would recommend it in future. Honestly, he did not think there was any better remedy than this, and the matter should be thoroughly ventilated.

In cross-examination by Mr. Holman Gregory, witness agreed that it was in the public interest to suppress quackery. Phthisis was not prevalent in his district. In some years he had three or four patients, in others ten or twelve. He could only remember two deaths among these patients, so it was difficult to say when a consumptive patient was quite cured. He believed this was a form of germicide. He would not recommend it for bronchitis. Not long ago a domestic servant in his employment had been taken ill. She was apparently cured, and was told she could get married. She died three weeks after her marriage.

Mr. Gregory: Is it your view that if this drug were used there would be no more consumption in the world?—I think it would go a long way towards stopping existing tuberculosis, and I think if this drug is given a proper trial by unbiassed medical men they will think the same as I do.

Frederick Cross (master mariner), the patient referred to by the last witness, and Mr. Meynor, L.R.C.P., having been called,

Mr. Campbell, L.R.C.S.Edin., L.S.A.Lond., said he had retired from practice this year, and had been the medical officer to the Post Office at Hastings and St. Leonards. He became qualified in 1864. He began to use Mr. Stevens's remedy in 1907, and had a distinct recollection of three cases. The first was a postman at Hastings, who began to spit blood in 1906, and he eventually suffered from consumption. He was sent to hospital, and was discharged apparently well. In 1907, however, he developed consumption of the windpipe. He (the witness) received a sample of the medicine from the plaintiff. He considered he was justified in using it on the first favourable opportunity. As a result of it, the patient began to put on flesh and to get rid of his cough. In 1911 he had pleurisy and pneumonia, but he recovered, and in July, 1912, he was at duty and looking well. As to the second case, that was a woman known to witness for many years, who was in the last stages of consumption. Had the disease not been arrested she would have died. He gave her Stevens's remedy, as a result of which she was able to leave her bed, and in June last she was still attending to her duties. The third case was that of a girl who had chronic phthisis in the left lung and tubercle of the throat. He had also successfully treated her with the plaintiff's remedy. He had a favourable opinion of the medicine, and would use it again. [The Court adjourned at this point.]

AGREEMENTS NOT TO PRACTISE.

W. H. L. inquires whether an agreement not to practise within so many miles and for so many years is binding on an assistant who has signed it. He has been told that if the latter chooses later to disregard the agreement there is no legal redress.

*** If the agreement has been properly drawn up and is a legal instrument, it is quite as valid as any other legal agreement, whether made with an assistant or locumtenent. There may be difficulties in the way of enforcing an agreement, according to circumstances, but they equally affect all agreements. All legal agreements can be enforced at law, and are binding.

According to the return just issued showing the rates of duties, taxes, or imposts collected by Imperial officers during the year ending March 31st, 1912, the net receipts for patent medicine stamps was £327,856 14s. 4½d., being an increase of £2,210 over the preceding year.

Medical News.

A meeting will be held in the Common Room of the British Medical Association, 429, Strand, W.C., on Thursday, October 31st, at 3 p.m., to discuss, in view of the forthcoming Representative Meeting, the principle underlying the Public Medical Service Schemes C and D (Supplement, October 12th)—namely, co-operation between the profession and approved societies. Any member of the Association interested is invited to attend. Dr. H. D. Ledward, Letchworth, Herts, is acting as convener.

The regular classes at the North-East London Post-Graduate College will be resumed next Wednesday, October 30th, when an address on lymph lavage as a therapeutic measure will be given by Sir Almroth Wright, F.R.S.

The annual meeting of Fellows and Members of the Royal College of Surgeons of England will be held at the College, Lincoln's Inn Fields, W.C., on Thursday, November 21st, at 3 p.m., when the annual report of the council will be laid before the meeting.

The departments of bacteriology and public health of King's College, London, have been transferred from the College in the Strand to the University laboratories, 62, Chandos Street, London, W.C., in the buildings constituting the medical school of the Charing Cross Hospital.

A recent addition to the list of *Black's Modern Guides* is one dealing with *Harrogate*, edited by Mr. Gordon Home. It contains many maps, and the charms of the spa and its neighbourhood are shown in twelve illustrations in colour. Reference is facilitated by placing the data under certain alphabetical headings, such as motoring, geology, cycling, baths, history, places of interest, and the like. It is published at the same price as the earlier volumes—namely, 1s. net.

Chirnside, in Berwickshire, was *en fete* on October 11th, the occasion being a presentation to Dr. Samuel Macvie, J.P., of a silver loving cup and a purse of sovereigns. Before the presentation was made there was a kind of masque, during which were successively described in verse Dr. Macvie's claims upon public affection, first as a physician, then as a musician and artist, and finally as the genius of Chirnside. Dr. Macvie went to reside in Chirnside some thirty-six years ago, and last year became a justice of the peace. When alluding to this gathering, the *Berwickshire Advertiser* described Dr. Macvie as an excellent prototype of Ian MacLaren's Drumtochty doctor.

Dr. Hugh Walsham, who has been associated with the electrical department of St. Bartholomew's Hospital for some sixteen or seventeen years, assumed formal charge of it last week in succession to Dr. Lewis Jones. The radiotherapy work of the institution has grown so greatly in the last few years that the number of rooms devoted to it in the new out-patient department is to be increased, and the muscle testing, electric bath treatment, and other measures which at this particular institution are classed under the heading of electro-therapeutics, are to be moved elsewhere. In the radiological department Dr. Walsham will have two chief assistants, two clinical assistants—both qualified men—and a number of clerks.

A quarterly court of the directors of the Society for Relief of Widows and Orphans of Medical Men was held on October 9th, the Right Hon. Sir Thomas Boor Crosby, Lord Mayor, president of the society, in the chair. Seventeen directors were present. Three gentlemen were elected members of the society. The sum of £514 was voted to be distributed at Christmas amongst the annuitants of the charity, each widow to receive £10, each orphan £3, and each orphan on the Copeland Fund £10. This is in addition to the half-yearly grants made in January and July. The society only grants relief to the widows and orphans of its deceased members, and the secretary is constantly receiving letters from widows of medical men asking for relief, but this has to be refused, as their husbands had not joined the society. Membership is open to any registered medical practitioner who at the time of his election is residing within a twenty-mile radius of Charing Cross. The annual subscription is two guineas, and there are special terms for life membership. The invested funds of the society now amount to £101,600. Further particulars or application forms for membership may be obtained from the secretary at the offices of the society, 11, Chandos Street, Cavendish Square, W.

Letters, Notes, and Answers.

QUERIES.

R. L. asks for experience of soamin in the treatment of phthisis and other forms of tuberculosis, and what is a good initial dose to use.

Income Tax.

S. W. commenced practice in February, 1911, and is assessed on £200. His takings in cash were £175 in 1911, and £100 to date in 1912.

*** The basis of assessment for the year 1912-13 in such a case is the amount of the net profits of the first year of the practice. On the figures given our correspondent should have no difficulty in proving that he made little, if any, profit in the first year. He should send to the surveyor of taxes an account showing the gross fees earned in that year and the professional expenses occurred, including a reasonable estimate of doubtful debts.

I. G. F. sold his car last year for £97 (the original cost having been £120) and bought a new one for £385. The surveyor refuses to allow as an expense a greater sum than £23, the difference between £120 and £97.

*** Our correspondent, on the strength of an article in the Journal of October 7th, 1911, claimed as a deduction £288 (the difference between the £385 and £97), as representing the full cost of the old car. This was clearly wrong, since the original cost of the old car was £120. In our opinion, the surveyor's contention is equally wrong. "H. G. F." is entitled to charge so much of his expenditure as represents the cost of renewal of his car. His expenditure is £288 (£385, less £97), and of this sum £120 is a repetition of the expenditure incurred when the first car was purchased, and £120 is, in our opinion, the amount proper to be allowed on account of the renewal. The point is one that is not dealt with specifically in the Income Tax Acts, and one that is not covered by any case decided in the courts. Since the expense is allowed as an expense for the year in which it is incurred, the effect will, on account of the operation of the three years' average, be to reduce the current year's assessment by only one-third of the sum allowed.

LETTERS, NOTES, ETC.

Hunterian Society's Medal.

In the paragraph published last week, p. 1087, regarding the Hunterian Society's medal, it should have been stated that it is awarded for the best essay by a general practitioner embodying the results of his own observations.

The Rule of the Road.

Dr. Chas. Mercier (Parkstone) writes: Sad experience has bred in me a wholesome reluctance to question your more than encyclopaedic knowledge, but on this occasion I must stick to my guns. If, before the ordonnance of 1722, there had been a custom of the road to keep to the left, the City Corporation would surely have ordered this custom to be observed, and would not have promulgated the order in terms of east and west; nor would my authority, whose name I have unfortunately forgotten, have had any occasion to assert, forty years afterwards, that the rule was still observed on London Bridge.

The City of London undoubtedly had then, as it has now, more traffic than any other place in the kingdom; and any custom with respect to traffic would be more likely to originate there than anywhere else. Now, we have it on unimpeachable authority that in the City of London the streets were so broad and the lanes so narrow, that on an important occasion the only practicable vehicle was a wheelbarrow. Westcheap and Eastcheap, the two considerable thoroughfares, were wide enough for a dozen carriages abreast, even after allowing for the booths that occupied their margins; there was no need for a rule of the road in them. Scarcely any of the remaining streets were wide enough for more than one cart, as any one familiar with the by-streets of the City knows; there was no possibility of a rule of the road in them. Old London Bridge, a long thoroughfare without side streets, was just wide enough for two vehicles to pass, and it was on the bridge that the need for a rule would first be felt. We might infer, therefore, even without the positive record that it did so, that the rule would be more likely to originate on London Bridge than in any other place in the kingdom. It must have originated somewhere.

The rule of the road at sea is statutory: the rule of the road on shore is not statutory: it is common law; that is to say, it is custom enforced by the courts. It possessed no legal force until the courts began to enforce it. When was the first decision? I do not know, and I doubt if even you do, but I hazard the assertion that it was subsequent to 1760—certainly subsequent to 1722. *Will you bet sixpence?*

Your assertion that all drivers hold the reins in the left hand merely shows how fleeting and unreliable is the human memory. I am old enough to remember the time, before the luggage tricycle was invented, when butcher boys drove ponies, and drove them uncommonly fast, and on the whole very well. I suppose that one motor bus will sacrifice more lives, on a single journey, than all the butcher boys in London took in a twelvemonth. These experts invariably drove standing up in their carts, with a rein in each hand, the hands a yard apart. I do not commend the practice for imitation, but it disposes of your assertion.

While I am upon this subject, I may advert to the fact that Dr. Lettsom, whether or not he was witty in himself, was certainly the cause of wit that was in other men. He was the provocation to the epigram, better known than the one you quote—

> When folks is sick they comes to I,
> I physicks, bleeds, and sweats 'em:
> And if in spite of that they die,
> What's that to I? I. Lettsom.

Dr. Major Greenwood (London) writes: Dr. Mercier's remarks in your last issue are very interesting, but I cannot think that the rule to which he refers took origin at the date he mentions, 1722. There was in all probability such a rule in existence long before then. That the rule originated in London, as far as this country is concerned, is very probable, and may have been originally derived from Roman sources, when London was a Roman municipium. Sir Lawrence Gomme has shown that there has been a continuity from Roman times with regard to many old customs that are directly traceable therefrom, and this may be one of them. It is true that the contrary rule holds good on the Continent, but some of the old London customs may reflect ancient Roman usages more correctly than Continental rules. It is not quite correct to say that our rule of the road applies to the United States universally, as I believe in some States the Continental rule applies. I am not sufficiently familiar with the civil law to be able to show what the old Roman rule was, although I have made searches therein, but I should think an answer might be found there. If our rule of the road is identical with the old Roman rule, it seems to me it would be a highly interesting fact, and would furnish further evidence of the truth of the continuity of Roman institutions in the history of ancient London.

Dr. S. D. Clippingdale (London) writes: With regard to the interesting letter from my friend, Dr. Mercier, upon the rule of the road, and your valuable comment upon that letter, may I give the reason held by some as to the varying rules of the road here and upon the Continent? If a sculpture or a fresco of a Roman chariot race be observed, it will be noticed that the chariot drivers keep to the left as we do in England. It is therefore supposed that the Romans introduced this rule of the road into this country. When the Huns overran Europe, it is supposed they brought with them the rule now in vogue on the Continent—namely, that of keeping to the right. The Huns never crossed the Channel, hence the English have retained the rule introduced when England was a Roman colony.

"Honour a Physician."

A Grateful Patient writes: I happened to be in the City on Friday, and, as the great bell of St. Paul's warned me that service was about to commence, I entered the Cathedral. It was the Festival of St. Luke, "the Beloved Physician," and the first lesson was so strikingly applicable to the present relations between the medical profession and the State that I ask you to print it, in the hope that it will meet the eye of Mr. Lloyd George, who is very fond of quoting Scripture. It is taken from the 38th chapter of Ecclesiasticus, and is as follows:

> Honour a physician with the honour due unto him for the uses which ye may have of him: for the Lord hath created him. For of the most High cometh healing, and he shall receive honour of the king. The skill of the physician shall lift up his head: and in the sight of great men he shall be in admiration. The Lord hath created medicines out of the earth; and he that is wise will not abhor them. Was not the water made sweet with wood, that the virtue thereof might be known? And He hath given men skill, that He might be honoured in His marvellous works. With such doth He heal men, and taketh away their pains. Of such doth the apothecary make a confection; and of his works there is no end; and from him is peace over all the earth. My son, in thy sickness be not negligent; but pray unto the Lord, and He will make thee whole. Leave off from sin, and order thine hands aright, and cleanse thy heart from all wickedness. Give a sweet savour, and a memorial of fine flour; and make a fat offering, as not being. Then give place to the physician, for the Lord hath created him; let him not go from thee, for thou hast need of him.

I was the more impressed by these words, as I have recently recovered from a very serious illness.

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